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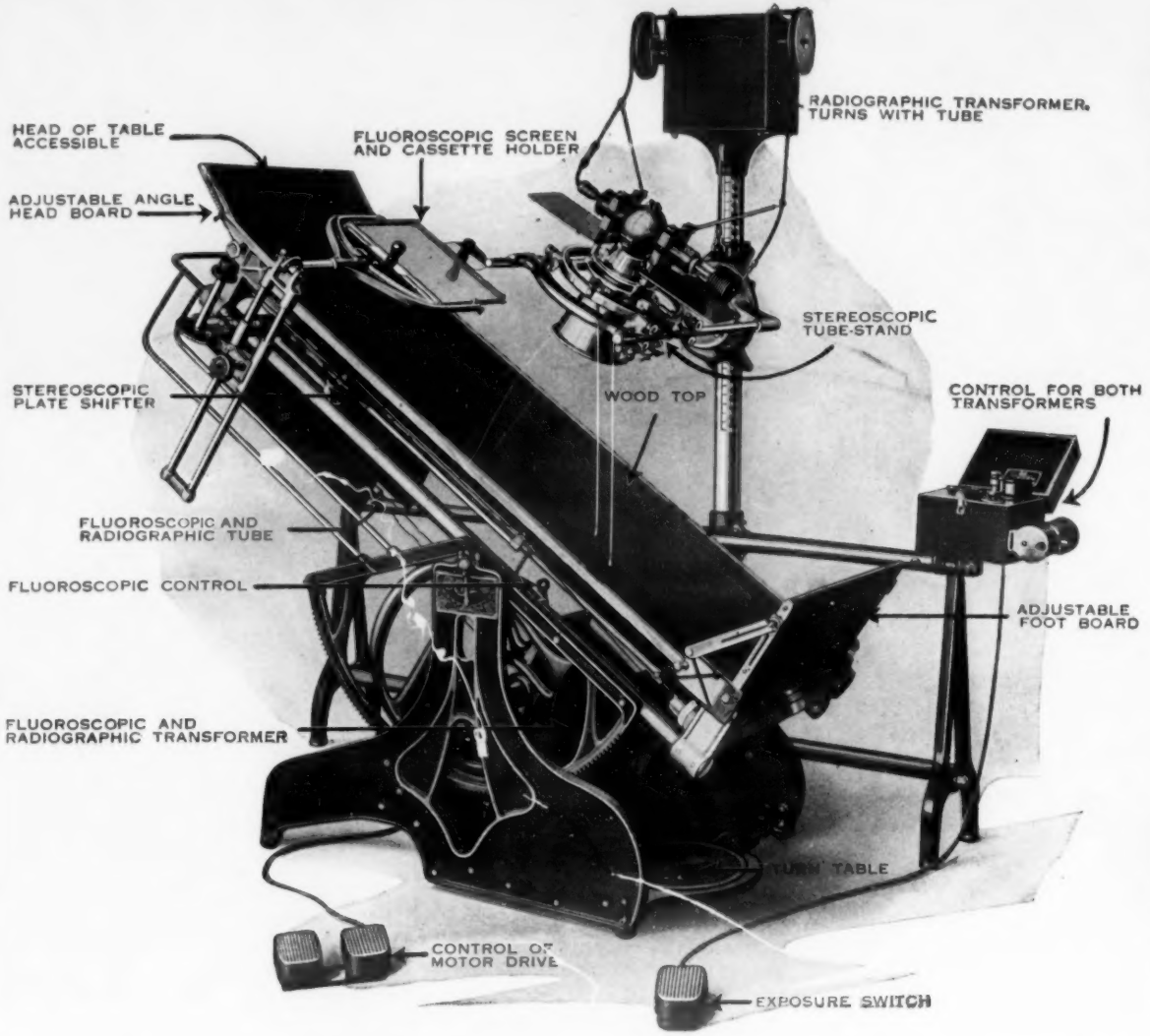

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THE AMERICAN JOURNAL OF ROENTGENOLOGY

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VEILS IN THE RIGHT HYPOCHONDRIUM AND THEIR DIFFERENTIATION FROM OTHER ORGANIC LESIONS AND SPASM*

BY LEWIS GREGORY COLE, M.D.
NEW YORK CITY

THE roentgen findings and the clinical symptoms of congenital or acquired veils in the right hypochondrium simulate those of gastric cancer, gastric ulcer, or postpyloric ulcer or pylorospasm to such a degree that they demand special consideration; first, because their recognition will materially reduce a percentage of erroneous roentgen diagnoses; and secondly, because some of these veils are of sufficient clinical significance to demand surgical procedure. All these conditions deform the cap, stomach, or duodenum, hence the characteristic roentgen findings of gastric cancer, gastric ulcer, postpyloric ulcer and pylorospasm have been more or less accurately described in previous articles. Time will not permit of their reiteration here. But an attempt will be made to describe the roentgen findings of these congenital or acquired veils.

Veils in the right hypochondrium have been recognized by surgeons and pathologists and described in various terms. Morris in 1905[1] referred to them as "cobwebs in the attic" and Harris in 1914[2] described them in a more scientific but less picturesque term, and his friends have connected his name with this condition, namely, the Harris membrane. Morris considered that this condition, which he referred to as "cobwebs in the attic" was of "gall spider" origin and therefore an acquired pathological condition caused by the penetration of toxin through the thin wall of the gall-bladder and its ducts. He states: "When there is an infection on a mucosal side of these thin-walled structures, toxins penetrate the walls in sufficient force to cause a toxic desquamation of the endothelium on the peritoneal side, endothelial cells are shed, plastic lymph exudes, coagulates, and is replaced by connective tissue" —and "webs of adhesions are spun," from the biliary tract to the adjacent hollow and solid viscera.

Morris evidently intended to differentiate these "cobwebs in the attic" from the dense inflammatory adhesions associated with an acute, subacute, or chronically inflamed gall-bladder, or one that contained stones or thick inspissated bile that could not be expelled. My reason for believing this is the following. Morris says: "Many a surgeon has been nonplussed by operating for gall-stones, finding no stones, and yet having the patient make a remarkable gain in general health, the fact that these adhesions were separated, incidentally, having been overlooked as a factor." He further states: "It is safer not to speak of gall-stones anyway, speak of choleocystitis." If this is the surgeon's privilege, why not the roentgenologist's?

In this article we will only consider the veil, or membrane, or cobweb adhesions in the right hypochondrium which occur in cases where the pathological condition of

*Read at the Twenty-Second Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Washington, D. C., Sept. 27–30, 1921.

[1] *Am. Med.*, July 15, 1905, x, No. 3, 95–97.

[2] *J. Amer. Med. Assn.*, April 1, 1914.

the gall-bladder *is not sufficiently extensive* to enable the surgeon to detect it by inspection or palpation of the gall-bladder and where the gall-bladder does not contain stones or inspissated bile which cannot be expelled, that is, in those cases where the gall-bladder is apparently normal. Harris recognized veils in the right hypochondrium; and in the first case operated on he considered that it was of inflammatory origin resulting from gall-bladder infection, although the bladder contained no stones and was apparently normal. Subsequently, from a study of six operative cases and an extensive study of embryological development of the peritoneum in this region, he concluded that this veil or membrane was an abnormal fold of the anterior mesogastrium, this mesogastrium being what medical students of my day knew as the "gastrohepatic ligament." In his article Harris graphically describes the embryological development of the peritoneum in this region and recites the clinical symptoms of these six cases and describes the roentgen findings.

The veil or membrane which Harris describes extends from the gall-bladder to the descending duodenum, or from the hepatic flexure of the colon to the descending duodenum.

Harris' reference to the roentgen diagnosis is as follows: "The use of the roentgen ray with the bismuth meal is a very valuable aid in the diagnosis, the long large caput showing a dilated first and perhaps a second portion of the duodenum with a fixed point at the right of the constriction." I believe that these roentgen findings described by Harris were what he thought they should be rather than what they really are, or, if we are to take him absolutely at his word both as to the location and extent of the veil which he described, the roentgen findings would then coincide with this condition; but, veils of the particular type and of the extent that he described form such a small proportion of the veils in the right hypochondrium that one would not be justified in applying his description or name to them; but I do believe that great credit should be given to him for drawing attention to the fact that at least some of these veils are of congenital origin.

During an intensive roentgenological study of this region of the alimentary tract, many cases of cap, stomach, and duodenal deformities were observed which were not characteristic of cancer, ulcer, postpyloric ulcer, pylorospasm, or of definite cholecystitis, with or without stones, and these simulated each other to such a degree that it was evident that these roentgen findings were all caused by the same or similar conditions. In hunting for a pathological condition to account for this group of roentgen findings I ran across Harris' article, and in spite of the roentgen findings which he described and his pathological description of the veil I have often used his name in describing them; but from a more careful study of his article, particularly if we accept his roentgenological findings, one is compelled to admit that the roentgenological and surgical findings of my cases do not coincide with those which he described.

I am convinced that Morris' "cobwebs in the attic" of "gall spider" origin and the abnormal fold of the anterior mesogastrium, part of which was described by Harris, are so similar that they cannot all be differentiated from each other either by roentgenological examination or by surgical exploration, but whether the etiological factor is a "gall spider" or some prehistoric monkey is of little interest to me; therefore, this condition will be referred to as a veil, congenital or acquired. I believe that both Morris and Harris were right in concluding that this veil causes symptoms. Whether or not surgical procedure is necessary in these cases depends on the severity or persistency of the symptoms; and whether or not permanent relief is obtained by surgical procedure is not within the scope of this article. Too short a time has elapsed since these cases have been operated upon, to determine this, but immediate relief has been obtained in most of the cases, and one of the cases has been free from symptoms for about six years.

The roentgen findings which are characteristic of a veil in the right hypochondrium are as follows:

1. The cap and perhaps the extreme pyloric end of the stomach are partly constricted or compressed, and fail to fill

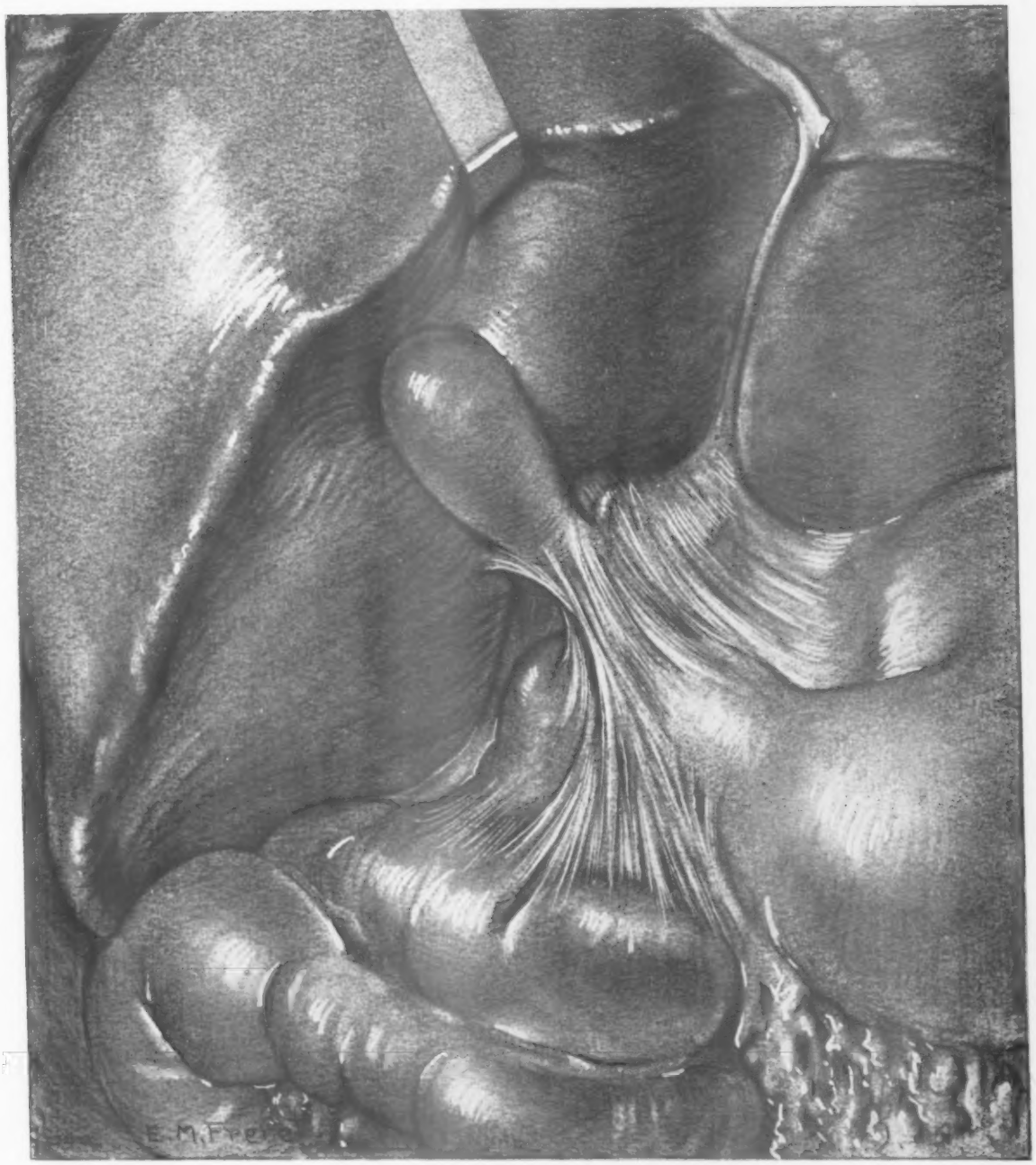

FIG. 1. Sketch of anatomical specimen showing a veil in the right hypochondrium involving: 1, the cap; 2, the neck of the gall-bladder; 3, the descending duodenum; 4, the pyloric end of the greater curvature of the stomach; 5, the hepatic flexure of the colon.

FIG. 1. Plan d'un spécimen anatomique démontrant à la vue une voile dans le hypochondre droit comprennant; 1, le bulbe duodénale; 2, le cou de la vésicule biliaire; 3, le duodénum descendant: 4, l'extrémité pylorique de la courbure plus grande de l'estomac; 5, la flexion hépatique du colon.

FIG. 1. Esquema de espécimen anatómico demostrando un velo en el hipocondrio derecho, que envuelve: 1, el bulbo duodenal: 2, el cuello de la vesícula biliar: 3, el duodeno descendiente: 4, el extremo pilórico, de la gran curvatura del estómago: 5, el asa hepatica del colon.

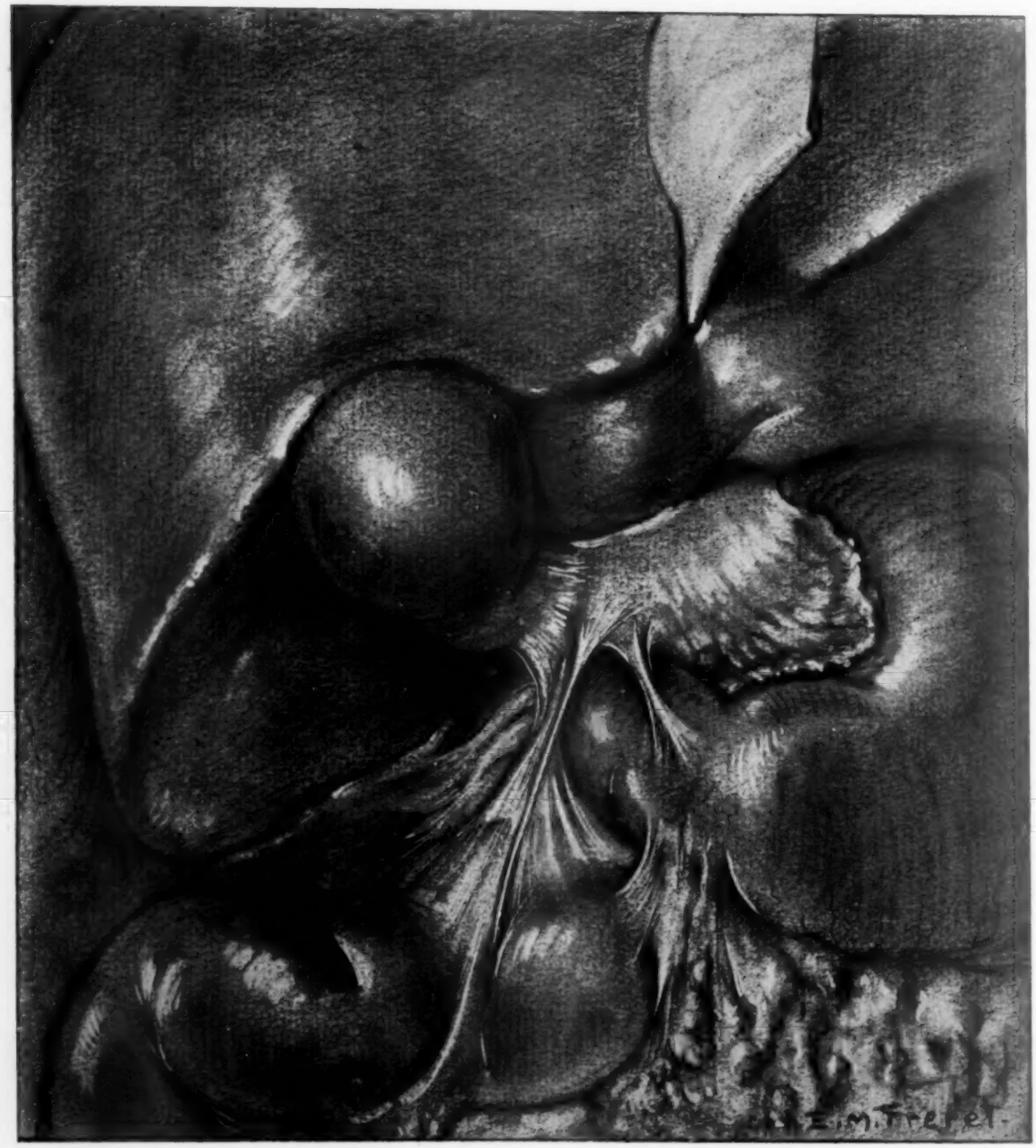

FIG. 2. Sketch of anatomical specimen (female, aged seventy) showing a veil involving: 1, the cap; 2, the pyloric end of the stomach; 3, the gall-bladder; 4, the hepatic flexure of the colon.

FIG. 2. Plan d'un spécimen anatomique (une femme ayante soixante-dix ans) démontrant à la vue d'une voile comprenant: 1, le bulbe duodénale; 2, l'extrémité pylorique del'estomac; 3, la vésicule biliaire; 4, la flexion hépatique du colon.

FIG. 2. Esquema de especimen anatómico (mujer de 70 años) mostrando un velo que envuelve: 1, el bulbo duodenal: 2, el extremo pilórico del estomago: 3, la vesícula biliar: 4, el asa hepàtica del colon.

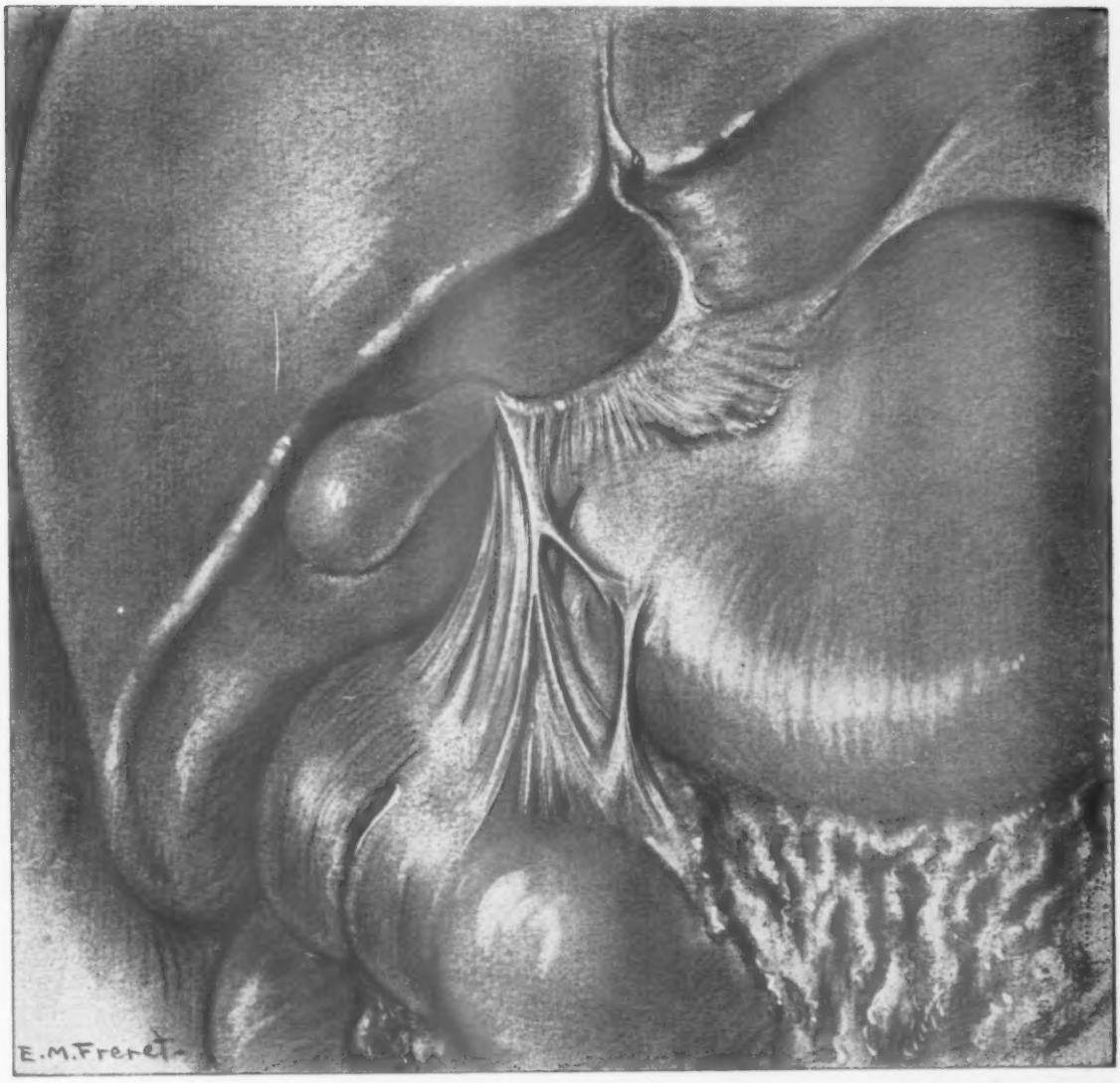

FIG. 3. Sketch of anatomical specimen (boy, aged seventeen) showing a veil involving: 1, the cap (slightly); 2, the pyloric end of stomach; 3, the cystic duct; 4, the hepatic flexure of the colon.

FIG. 3. Plan d'un spécimen anatomique (un garcon ayant dix-sept ans) démontrant à la vue d'une voile comprennant: 1, le bulbe duodénale (un peu); 2, l'extrémité pylorique de l'estomac; 3, le canal cystique; 4, la flexion hépatique du colon.

FIG. 3. Esquema de espécimen anatomico (varon, de 17 años) mostrando un velo que envuelve: 1, el bulbo duodenal (ligeramente): 2, el extremo pilorico del estomago: 3, el conducto cistico: 4, el asa hepática del colon.

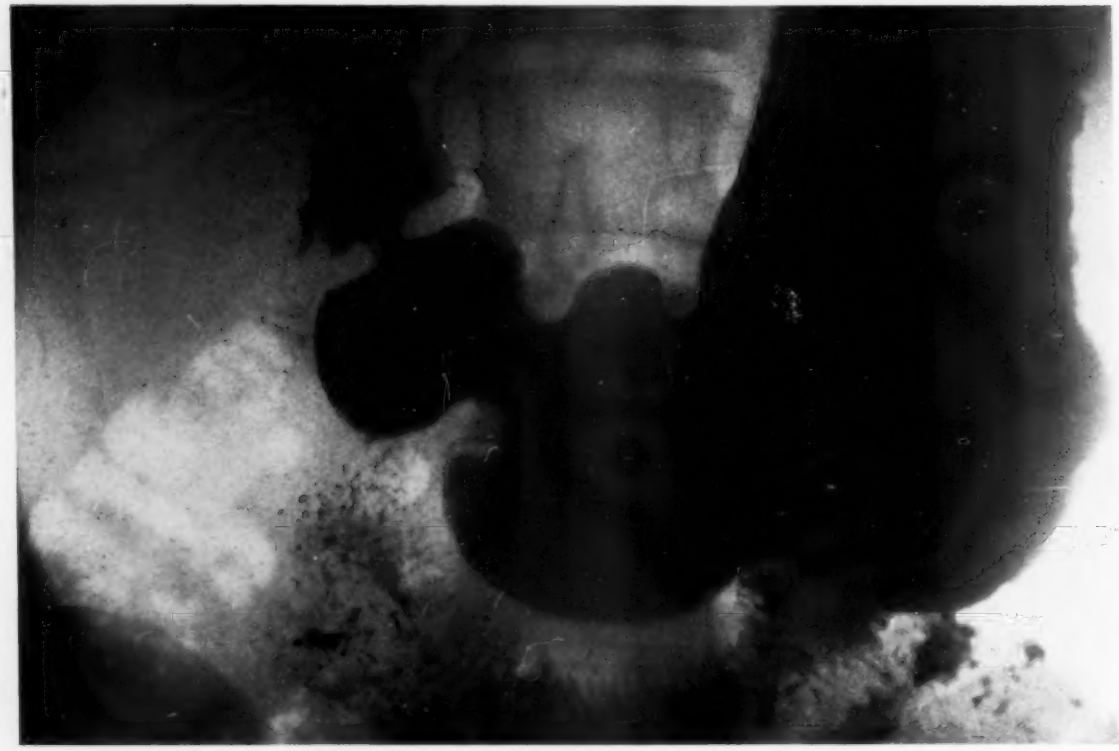

FIG. 4 and 4′. Illustrates a veil that involves the outer surface of the cap, the gall-bladder and hepatic flexure of the colon filled with gas.

FIG. 4 et 4′. Démontrant une voile qui comprend la surface extérieure la du bulbe duodénale, vésicule biliaire et la flexion hépatique du colon emplie par le gaz.

FIG. 4 et 4′. Ilustrando un velo que envuelve la cara externa del bulbo y duodenal, la vesícula biliar y al asa hepática del colon llenos de gas.

FIG. 5 and 5′. Illustrates a veil that involves the anterior surface of the cap and hepatic flexure of the colon.
FIG. 5 et 5′. Démontrant une voile qui comprend la surface antérieure du bulbe duodénale et la flexion hépatique du colon.
FIG. 5 et 5′. Ilustrando un velo que envuelve la cara anterior del bulbo duodenal, y el asa hepática del colon.

FIG. 6 and 6′. Illustrates a veil that involves the lesser curvature of the stomach and the superior surface of the cap.
FIG. 6 et 6′. Démontrant une voile qui comprend la courbure moindre de l'estomac et la surface supérieure du bulbe duodénale.
FIG. 6 et 6′. Ilustrando un velo que envuelve la curvatura menor del es, tómago y la cara superior del bulbo duodenal.

FIG. 7 and 7′. Illustrates a veil that involves the anterior surface of the cap, anterior surface of the descending duodenum and the hepatic flexure of the colon. The gall-bladder indenting the upper surface of the descending duodenum is also probably involved in this veil.

FIG. 7 et 7′. Démontrant une voile qui comprend la surface antérieure du bulbe duodénale, la surface antérieure du duodenum descendant et la flexion hépatique du colon. La vésicule biliaire poussant la surface supérieure du duodenum descendant est probablement aussi affectée dans cette voile.

FIG. 7 et 7′. Ilustrando un velo que envuelve la cara anterior lde bulbo duodenal, la cara anterior del duodeno descendiente y el asa hepatica del colon. La vesicula biliar, indentando la cara superior del duodeno descendiente, está probablemente tambien en este velo.

FIG. 8 and 8′. Illustrates a veil that involves the anterior surface of the cap. The deformity of the cap so closely resembles the deformity of a post pyloric ulcer that the differential diagnosis is very difficult.

FIG. 8 et 8′. Démontrant une voile qui comprend la surface antérieure du bulbe duodénale. La déformité ressemble tellement la déformité d'un ulcère post-pylorique que le diagnose différentiel est bien difficile.

FIG. 8 et 8′. Ilustrando un velo que envuelve la cara anterior del bulbo duodenal. La deformidad del caput asemeja de tal modo a la deformidad de una úlcera posterior del píloro que hace muy dificil el diagnostico diferencial.

to their normal dimensions, particularly on the anterior superior surface (Fig. 4).

2. The left superior surface of the cap has a thin feathered-out appearance, whereas the right side of the cap has a clear-cut, definite, well-defined line (Fig. 5).

3. If the veil also involves the extreme pyloric end of the lesser curvature of the stomach, this region has a puckered appearance, somewhat simulating the finding observed in cases of prepyloric folds (Fig. 6).

4. The pyloric sphincter is clear cut and well defined if the veil involves only the cap. But if it also involves the stomach, the pyloric sphincter, particularly on the lesser curvature surface of the lumen, is irregular and often thickened by comparison with the sphincter on the opposite side of the lumen (Figs. 5 and 6).

5. The deformity is more marked with the patient in the erect or prone posture, and may be absent with the patient lying on the right side.

6. The line or torsion runs downward and to the right from the gastrohepatic ligament (mesogastrium) toward the gall-bladder, the under surface of the liver, or the hepatic flexure (Figs. 4, 7, and 8).

7. These veils are rarely if ever obstructive to the pylorus, and if gastric retention occurs, it is probably functional rather than organic.

8. If the veil fails to extend as far up as the cap, or stomach, it may then involve only the anterior surface of the descending duodenum, causing that to be adherent to the under surface of the liver, gall-bladder, or hepatic flexure. This is the type of pathology described by Harris, but the veil in Figure 7 also extends up across the cap. I have seen no case where the veil is limited to the descending duodenum.

9. There may be an angulation, rarely amounting to a partial obstruction, involving the midportion of the descending duodenum; it is evident that this was accentuated to such a degree in the cases described by Harris that he did not realize that this veil extended up to and across the anterior surface of the cap; or, perhaps in his case, the cap was not involved in this veil (Fig. 7).

These veils, either by direct contraction

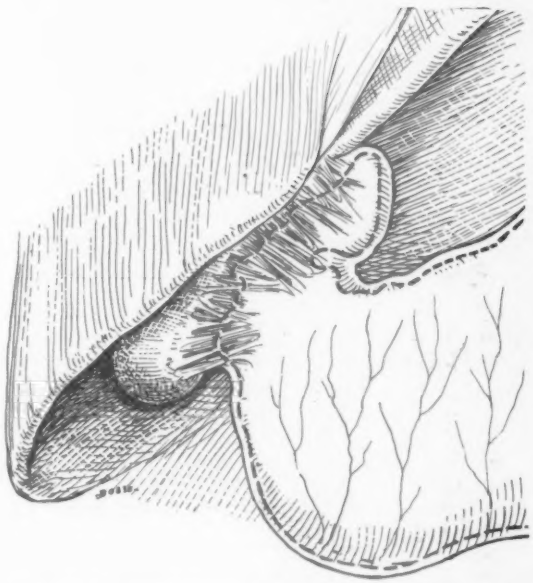

FIG. 9 and 9'. Illustrates a veil that involves the right side of the cap, the pyloric end of the stomach and the gall-bladder in such a manner that it cannot be determined whether this is of gall spider origin or a congenital membrane.

FIG. 9 et 9'. Démontrant une voile que comprend le côté droit du bulbe duodénale, l'extrémité pylorique de l'estomac et la vésicule biliaire d'une telle façon que l'on ne peut determiner si l'origine est la vésieule biliaire ou une membrane congénitale.

FIG. 9 et 9'. Ilustrando un velo que envuelve el lado derecho del bulbo duodenal, el extremo pilorico del estomago y la vesicula biliar de tal manera que no puede determinarse si su origen es secundario a inflamacion de la vesicula biliar. o es una membrana congenita.

or by tortion with the patient in certain postures, cause a deformity of the hollow viscera in the right hypochondrium, particularly the cap. This results in roentgen findings which so closely simulate either postpyloric ulcers or gall-bladder adhesions, that it probably accounts for most of the erroneous roentgen diagnoses of postpyloric ulcers, especially in those cases where a sufficiently extensive series of plates has been made and carefully studied by a competent observer. (Figure 8 closely resembles postpyloric ulcer; Figure 9 simulates gall-bladder adhesions.)

Unfortunately, when such an erroneous roentgen diagnosis is made and surgical procedure results therefrom and no ulcer or pathological gall-bladder is found, the surgeon, unless he is familiar with these veils or membranes and knows how to look for them, will fail to recognize them and will refer to the case as an absolute roentgenological error, when in fact a pathological condition exists which could and should be recognized and relieved by surgical procedure, thereby rendering the roentgen diagnosis only partly erroneous, in that it showed a pathological process which was not properly differentiated from ulcer or gall-bladder adhesions.

In several instances I have reported such veils as of unknown origin, and on one occasion the surgeon insisted on knowing whether I thought they were of gall-bladder origin or the result of a postpyloric ulcer. I insisted that as far as I was concerned, they were unknown. At operation the veil was found like a group of silver threads; there was no ulcer or gross evidence of gall-bladder pathology; and when I turned the tables on the surgeon and asked him what the origin of this veil was, he said, "I don't know what caused it"—it, therefore, remained of unknown origin.

The symptoms associated with these veils are often out of all proportion to the extent of the pathology. It seems that when these membranes are fairly local and involve the cap and stomach in close proximity to the pyloric sphincter, perhaps because of the nerve supply or because of local torsion, the pain—a stitching or burning pain—is most frequently felt, especially when the patient is in the erect posture. It is often relieved by lying down and often associated with a meal because the weight of the full stomach will drag on the veil or membrane. In other cases, especially where this veil extends to the hepatic flexure, it is associated with a full ascending or transverse colon and may then be temporarily relieved by catharsis.

While it is possible in many cases to make a positive differential diagnosis between cancer, ulcer, gall-bladder adhesions, spasm, and this veil or membrane, yet there are cases where these conditions so closely simulate each other that I have allowed myself the privilege, in about 10 per cent of the cases, of reporting the roentgen findings in the following manner: "One is justified in making a positive diagnosis of an organic lesion, not a spasm, involving the cap, or stomach, or duodenum; this may be due either to an ulcer, gall-bladder adhesions, or a veil, the weight of evidence being in favor of the one which it most closely resembles." In such cases the surgeon should be forewarned and go armed in the search of a veil, especially if he does not find an easily recognized ulcer or pathological gall-bladder. An erroneous diagnosis is thereby turned into a correct one, the pathology detected, and the patient relieved of his symptoms.

Figures 1, 2 and 3 are sketches of anatomical specimens made for Dr. J. P. Hoguet at the Cornell Medical College.[3] These give one a very graphic picture of some of these types of veils in the right hypochondrium. These are accentuated by the manner in which the liver and gall-bladder are drawn up by the retractor in order that E. M. Freret, the artist, might get an adequate exposure. This retraction upward of the liver and gall-bladder makes the torsion appear to be upward and to the right, instead of down and to the right, as it would be if the liver was dropped back into its normal position.

Figures 4, 5, 6, 7, 8, and 9 are roentgenograms of various types of veils in the right hypochondrium and figures 4′, 5′, 6′, 7′, 8′ and 9′ are explanatory drawings made by Mr. K. K. Bosse. The dotted line is an

[3] Many thanks are extended to Dr. Walter Weller who observed these cases during his demonstration of anatomy at Cornell Medical College. An additional photograph of another specimen was sent to me by Dr. H. E. Schaef, London, Ontario, Canada.

exact duplicate of the corresponding roentgenogram of the stomach and cap. The liver, the gall-bladder, and, in some of the figures, the colon and duodenum sketched into the picture to show the relation of these viscera to the stomach and cap, and the drag that is caused by this veil.

All the accompanying lead pencil sketches are only diagrammatic; and for the sake of demonstration the liver is shown to be further away from the stomach than normal in order to demonstrate the line of the veil.

In Figure 4 the veil runs downward and to the right from the under surface of the liver and across the anterior surface of the cap and is adherent to the gall-bladder, and possibly the colon.

In Figure 5 the veil extends from the under surface of the liver to the right side of the cap, apparently not involving the gall-bladder, but extends down to the hepatic flexure of the colon.

In Figure 6 the veil runs from the under surface of the liver to the lesser curvature of the stomach and left side and superior surface of the cap. This is a rather broad veil.

In Figure 7 the veil runs from the anterior superior surface of the cap downward and to the right, across the anterior surface of the descending duodenum to the hepatic flexure of the colon. It may possibly extend as far up as the liver. The gall-bladder is observed lying against the upper surface of the descending duodenum.

In Figure 8 the veil involves the left superior surface of the cap, running downward to the colon, causing a constant indentation which so closely resembles induration of a postpyloric ulcer that one would not be justified in making a positive differentiation between a veil and a postpyloric ulcer. This is one of the 10 per cent cases where the cause of the deformity of the cap could only be determined by surgical exploration, and perhaps not even by that method.

In Figure 9 the veil extends from the under surface of the liver and gall-bladder to the right side of the cap and extreme pyloric end of the greater curvature of the stomach, drawing it upward and to the right. This type of pathology is, I believe, definitely of "gall spider" origin as described by Morris, and might readily be diagnosed definitely as gall-bladder adhesions. But if a perfectly normal gall-bladder were observed on surgical findings and these adhesions were not noted, the case would be reported as a roentgen error.

Considering that it requires as much surgical skill and experience to recognize these veils after the abdomen is opened as it requires roentgenological experience to interpret the findings before the abdomen is opened, and since this is a subject that has not been extensively considered either from a surgical or anatomical standpoint, it seemed wise to collaborate with some surgeon in the publication of a monograph on the subject, discussing it from its embryological, anatomical, clinical, roentgenological and surgical aspects.

Dr. J. P. Hoguet, after operating on several of my cases, became interested in this subject and has consented to collaborate in such a monograph which we hope to finish shortly.

DISCUSSION

DR. IMBODEN. I wish to thank Dr. Cole for presenting this paper. I have been interested in this question ever since 1914, and I wish to say "Amen" especially to two things he has said. First, that the thing is there and that it is comparatively frequent; and second, that you have got to insist that the surgeon find your lesion. There is one little point in technique in which I differ slightly from Dr. Cole; that is, I have been able to make diagnoses at the Presbyterian Hospital by use of the screen entirely.

DR. LEWALD. In a discussion of any of the pathological conditions about the duodenum, just as a matter of interest one should never forget the analogy of non-rotation of the colon in which case the duodenum also runs an abnormal course. I have known two operations to be performed on a diagnosis of duodenal lesion in which the condition found at operation was exactly as one would expect in this type of duodenal anatomy.

The duodenum merges with the jejunum without distinction of any first, second, or third portion. It simply passes off to the right side. If one is not familiar with this he might make an error in believing he is dealing with a pathological condition of the duodenum due to ulcer or adhesions.

DR. WHITE. Dr. Cole describes these findings which are very definite and very characteristic, and which occur, apparently, in the gall-bladder region, and which apparently join the cap to the colon or to the gall-bladder region. It seems to me very difficult to distinguish whether we are dealing with a truly jejunal veil or whether we are dealing with something left from previous gall-bladder involvement. It does not seem to me that this would rule out gall-bladder inflammation. There may be involvement of the gall-bladder, which may clear up for a time, and the surgeon finds the gall-bladder pretty normal, but the inflammation remains and leaves some adhesions in that region.

DR. COLE. I am thoroughly convinced that if a man wants to examine by fluoroscopic methods, he can do it. This may be the easier way. For some reason or other I do not take to fluoroscopy, and it can be done by roentgenological methods. I want to emphasize, however, that if you are going to do it by roentgenological method, do not do it by two or three plates. Fluoroscopy is better than two or three plates.

MINIMUM ERYTHEMA DOSE WITH DIAGNOSTIC VOLTAGES*

BY ARTHUR W. ERSKINE, M.D.

CEDAR RAPIDS, IOWA

A SEARCH of the literature shows that it is almost impossible to find two writers who agree as to the limits of safety in roentgenography. Interest in this subject has lately been stimulated by the popularity of the Potter modification of the Bucky diaphragm, requiring, as it does, a fixed target-skin distance, and longer exposures. It was with the hope of accumulating some accurate data that the experiments described in the following paragraphs were undertaken.

Using a Victor Universal Machine, and a standard broad focus Coolidge tube, the setting was found which would just break a gap of 30 mm. between two polished metal spheres with a diameter of 125 mm. This was found to give a constant reading of 58 on the kilovolt meter, and was assumed to indicate that the voltage was 60,000. Ten ma. were allowed to pass through the tube so that the lower scale of the milliammeter could be used, thus insuring greater accuracy. The milliammeter was tested against a standard one furnished through the kindness of Mr. G. H. White, and its accuracy found to be within the limits of permissible error. Upon measuring the thickness of patients weighing more than 200 pounds, it was found that in no case did the anteroposterior diameter exceed 17 inches without compression, or 15 inches with compression. I assumed that, with a target-plate distance of 25 inches, it would not be necessary to bring the tube nearer to the skin than 10 inches, and the experiments were conducted with a 10 inch target-skin distance.

By a simple arithmetical calculation based upon a supposedly known dosage, checked by comparative photographic measurements, it was estimated that at a target-skin distance of 10 inches, with no protecting filter, 800 ma. seconds would produce a visible erythema. When 1 mm. of aluminum is used as a filter, with a voltage of 60,000, the ratio of time between the filtered and unfiltered dose is as 7 is to 3; that is, in order to produce the same effect through 1 mm. of aluminum, the exposure must be two and one-third times as long. Accordingly, it was assumed that 1,800 ma. seconds would produce an erythema when 1 mm. of aluminum was used as a protecting filter.

The minimum erythema dose can properly be defined as the smallest amount of *x*-rays which will produce a visible reddening of the skin. To demonstrate that a given amount of *x*-rays is a minimum erythema dose, it must not only be shown that it will cause reddening of the exposed skin when compared to adjacent protected skin, but also that a smaller part of the same dose will not produce redness.

*Read at the Twenty-Second Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Washington, D. C., September 27–30, 1921.

Accordingly I exposed four areas, on the backs of each of the first group of patients, to unfiltered rays for 500, 600, 700 and 800 ma. seconds respectively, and four areas to rays filtered through 1 mm. of aluminum for 1,200, 1,400 and 1,800 ma. seconds respectively. In each case, all the areas became red, showing that the minimum erythema dose was 500 or less ma. seconds of unfiltered rays and 1,200 or less ma. seconds of rays filtered through 1 mm. of aluminum. Exposing the second group of patients, the length of the exposures was reduced to about half of that in the first group. All the patients showed a reaction in the areas exposed to 400 ma. seconds of unfiltered rays, and 800 ma. seconds of rays filtered through 1 mm. of aluminum. The skin of only one patient, a blonde, became red in the area exposed to 350 ma. seconds of unfiltered rays, and in no case did the skin exposed to less than 800 ma. seconds of rays filtered through 1 mm. of aluminum, show any redness. I concluded, therefore, that at 60,000 volts, with a 10 inch target-skin distance, an exposure of 400 ma. seconds is the minimum erythema dose, and that double this exposure must be used to produce the same effect through 1 mm. of aluminum.

Even with this apparently successful method of measurement, one must not forget that there is considerable variance in the estimation of erythema. The degree of reddening is a very potent factor. The susceptibility of the skin is also to be considered; as there are a few individuals, especially those of fair complexion, whose skins are much more sensitive to the *x*-ray than others.

I was very much surprised to note the slight difference between the appearance of the areas receiving the minimum erythema dose, and those receiving twice as much. I formerly believed that doubling the erythema dose would produce blistering. I find, in checking up, that what I have been calling a minimum erythema dose, is from one and one-half to twice that amount.

DISCUSSION

DR. HICKEY. I would like to ask Dr. Erskine how large an area he exposed.

DR. PIRIE. I would like to ask Dr. Erskine how many patients he tried this minimum erythema dose on. Imagine getting half a dozen patients all exposed at the same distance and same time. Would they all absolutely respond in the same way? Suppose you exposed half a dozen people in the sunshine for a day. Would they all respond in the same way?

DR. DARLING. I would like to supplement that question with one specifically raising the question of idiocyncrasies—whether Dr. Erskine finds anything in that.

DR. KREISEL. I would like to ask at which time—how many days or hours, after exposure was observation made and how long did the erythemas last.

DR. ULLMAN. I would like to know the size of sphere.

DR. ERSKINE (closing discussion). The size of the sphere was 125 mm.,—the standard size as given by the American Institute of Electrical Engineers. This work was started almost a year ago when we could not buy spheres. We had to have them made. A break gap at 30 mm. we assumed indicated that the voltage was 60,000. The size of the areas was 2 cm. They were square with lead protecting surrounding skin. The number of patients in the first group in which all areas became red, was six. Then we saw that we were wrong in our estimations, that we were giving none of them a minimum dose; and after correcting our technique we started again.

DR. PIRIE. How many patients were exposed the same day?

DR. ERSKINE. Four. These were not all patients. We used the girls in the offices on the floor and anybody who would serve. The part of the body used was the back. We checked that with treatment cases on the front. The earliest erythema appeared three days after treatment. Observations were made in about three or four days, a week, and in ten days. The slowest ones to appear were in ten days. The part of the back exposed was between the shoulders. About thirty patients in all were exposed. There was a little difference between blondes and brunettes.

DR. PIRIE. Did you find primary erythema and secondary erythema afterward?

DR. ERSKINE. In one patient only—and then very little. We saw very little variation in idiosyncrasies. One blonde reddened up in the area which was exposed 350 ma. seconds, but not in the one that was exposed 400 ma. seconds.

DR. PFAHLER. With 400 ma. seconds, how soon did this redness appear?

DR. ERSKINE (final closing). The average would be seven or ten days.

POSSIBILITIES OF ROENTGEN-RAY TREATMENT IN CANCER OF THE PANCREAS*

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THE intention originally in presenting a short paper on this subject was to link together chronic pancreatitis with cancer of the pancreas, in view of the more than merely theoretical grounds which we have for believing that a rather definite relation exists between the two. Regarding such a sequence Ewing[1] says, "Carcinoma of the pancreatic parenchyma may be regarded as beginning in a functional hyperplasia following cirrhosis and carcinoma of the ducts from chronic irritation and stasis in these canals." Such irritation and stasis is very commonly present, as we know, in many forms of gall-bladder infection, while chronic pancreatitis has frequently been reported as following upon cholecystectomy, although we have seen no such reports after adequate drainage of the gall-bladder and ducts.

Deaver[2] is authority for the belief that "the disease is secondary to the more common forms of abdominal inflammation, and infection is by way of the lymphatics, while obstruction plays little part." He, however, urges the importance of adequate drainage in the surgical treatment.

Nicoll[3] points out that "chronic pancreatitis is a disease of much more frequent occurrence than has been commonly supposed. It commonly complicates catarrhal affections of the biliary apparatus, of the duodenum and of the vermiform appendix but may exist as an affection standing alone."

These are merely a few of the many references available, but it is not our desire to pursue this side of the subject since we have no definite proof to offer that there is a sequence. The possibility, however, is sufficient to stimulate us to every effort in the direction of early and accurate recognition of the disease before the further progress shall have become established.

I shall, therefore, limit myself to the subject of cancer of the pancreas. Here, one is convinced there is room for more improvement in diagnosis. Cancer of the pancreas has been variously reported as ranging from 1.2 to 2 per cent as based upon autopsy findings. In the living subject it is certainly not recognized so accurately as we should expect with our present methods.

In the analysis recently published by Lafferty of 1,100 gastro-intestinal examinations he reports one case of cancer of the pancreas and no cases of pancreatic cyst, or chronic pancreatitis. Other publications make no reference to the subject as a finding in gastro-intestinal investigations. This led me to look up our own figures and I find that in the last 11,000 bismuth examinations at the Toronto General Hospital we have recognized prior to operation 10 carcinomas of the pancreas, 3 pancreatic cysts and 2 cases of chronic pancreatitis. This gives a figure fairly close to that of Lafferty but still far below the actual occurrence of the disease. With more thoroughness and more routine examinations it ought to be possible to recognize these conditions with much greater certainty than has hitherto been the case.

The importance of such early recognition, especially of cancer of the pancreas, takes on added emphasis if the prospect of successful treatment is not more than a fortunate coincidence in a few cases. It is largely with the hope of bringing to light other cases similar to our own and stimulating interest in the subject generally, that I have had the temerity to come before you with any report based upon so few cases.

In a review of the entire literature I have not succeeded in finding a single reference to *x*-ray or radium therapy in carcinoma of the pancreas, and yet if any

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considerable number of cases respond, as ours have up to the present, it is quite a possibility that this disease will be found to be more easily influenced than some other forms of carcinoma, particularly of the gastro-intestinal tract.

CASE I. Male, age fifty-two. Roentgen-ray examination on December 1, 1920, showed the characteristic wide sweep of the duodenum with restriction of the pyloric end of the stomach and great lack of pliability in the area included in the circle of the duodenum. A roentgen diagnosis was returned of pancreatic carcinoma, probably primary in the head of the pancreas.

At operation on December 4, 1920, this diagnosis was confirmed and a section removed, but no other surgical procedure was attempted.

Pathologist's report was adenocarcinoma.

The patient was then referred for deep therapy partly as a placebo, partly to relieve pain which was severe, and partly to test the possibilities of higher voltage treatment, as we had recently installed a machine giving a 12 inch gap.

The first series of treatments was commenced on December 10, 1920. At this time the patient was greatly emaciated, weighing only 91 lbs., and his general condition was so low that it was thought wise to administer a modified series. This consisted of three areas, each 6 in. square, over the front and sides of the abdomen. Each area received an exposure according to the following formula: Ma. 5, distance 12 in., spark gap 12 in. between blunt points, filters 10 mm. of aluminum + 1 cm. of sole leather; time forty-five minutes.

Following the treatments pain was entirely relieved almost at once. A brisk erythema ensued but without vesication, and it subsided promptly. Considerable clinical improvement occurred, and early in January a second series of six areas was given, the same formula being used as before. Two months later a third series of six areas was administered. At this time he had gained 29 pounds and was in every way wonderfully improved. This improvement has continued up to the present time. He now weighs 139 pounds, which he states is his normal weight, and has carried on his regular business actively all summer. Roentgen ray examination shows no evidence of any mass, and he appears to be free from active disease.

CASE II. There was a history of long-continued digestive disability with three abdominal operations, without relief. Roentgen ray examination shows a somewhat similar appearance to that described in Case I, but with much more irregularity and fixation. A roentgen diagnosis of pancreatic carcinoma was made, the abdomen again opened and the mass found. A section of this was reported as pancreatic adenocarcinoma. The mass was quite extensive, everywhere adherent, and, of course, inoperable.

This operation was performed July, 1919, but we did not get the patient for treatment until January, 1920, about the time the improvement in Case I began to be apparent.

Treatment was similar and need not be detailed. When it was commenced he was confined to bed, emaciated and steadily going downhill. Great pain was present and this again was promptly relieved by treatment. At the present time he is practically symptom-free, has gained 20 pounds in weight, and he, also, appears to have no active disease.

CASE III. This case was referred about the same time as Case II. No roentgen ray examination had been made, but at operation in another city a large mass had been found, apparently primary in the pancreas but involving by extension the entire pyloric end of the stomach and the liver and gall-bladder.

We administered one series of four areas, using the same formula as previously detailed. Five weeks later a severe hemorrhage occurred and masses were vomited, said to be cancer tissue, though none was saved for section. The patient died forty-eight hours following this; no autopsy. I have previously reported this case during a discussion and quoted it as a possible danger in intensive therapy with high voltages. Since looking up the literature more carefully, it appears that this should be regarded as one of the normal terminations of such extensive involvement. This

case was obviously too far advanced to offer any reasonable prospect of cure, and probably should not have been subjected to treatment.

Other cases have since been treated with gratifying preliminary results, but none is over six months' duration, and will not be referred to further at this time.

We, therefore, have 2 cases out of the 3 treated, which are apparently well, nearly a year after treatment. All 3 cases have been proven, by every means at our disposal, to be cancer of the pancreas. In both the living cases the condition was advanced so that the expectation of life was very short: Case I, probably only a few weeks, Case II, a few months at the most. In both these the immediate result of treatment was more prompt and complete than has been our experience in any other form of carcinoma. I, therefore, feel that there is something more than a mere coincidence and that we may possibly be dealing with a disease which is more susceptible to radiation than other forms. If this should prove to be the case, remembering the utter helplessness of surgery, and every other therapeutic agent, then we are presented with a great opportunity for service.

In any case the results up to the present time are sufficiently encouraging to justify us in drawing these three conclusions:

1. It is possible favorably to influence the growth of pancreatic cancer, and this is sufficient to justify the intensive irradiation of every case as early as the diagnosis can be established.

2. It appears probable that adenocarcinoma of the pancreas is more susceptible to irradiation than some other forms of adenocarcinoma.

3. Efforts should be made to perfect our ability to recognize the disease as early as possible.

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DISCUSSION

Dr. Holmes. I would like to know whether examination of the urine was made to determine the presence of sugar.

Dr. Richards (closing discussion). I could reply to Dr. Holmes' question by saying that examinations for sugar in the urine have been made. Sugar was present in the urine in both cases when treatment was commenced.

THE RELATION OF TEMPERATURE CHANGES TO ROENTGEN-RAY SKIN REACTIONS*

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In therapeutic work the erythema dose, is commonly referred to as a definite fixed entity often expressed mathematically; and yet most experienced roentgenologists who are constantly administering the same dose of rays to the skin of numerous patients must have been struck with the varying degrees of reaction obtained. This is particularly noticeable in clinics where deep therapy is done, since the effect of a single dose can be observed when the patient returns for further treatment. The attitude of the profession toward such variations during the past decade was aptly put by Gawalowski[1] who stated that the tendency to blame uncritically all untoward results of roentgen-ray therapy on idiosyncrasy soon met with opposition, and many authors went to the other extreme and denied the possibility of idiosyncrasy, explaining poor results by errors in dosage and imperfection of the methods used. MacKee[2] has seen fit to devote a chapter of his recent book to the subject of idiosyncrasy and has summed up the opinions of thirty-two

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of the leading roentgenologists and dermatologists of this country. The majority of these men believe that there is a definite variation in the susceptibility of the skin of a degree too small to be called idiosyncrasy.

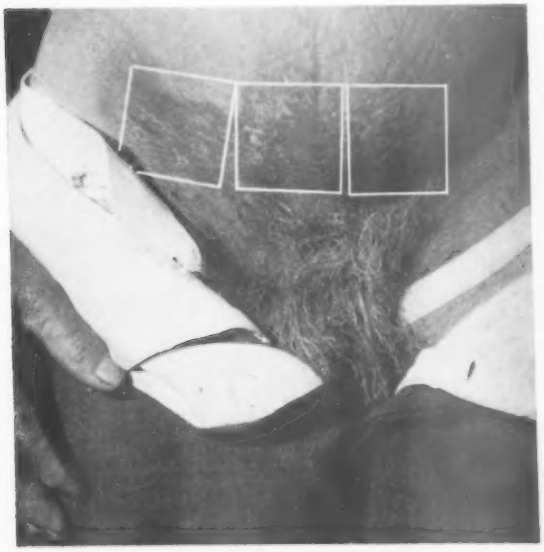

FIG. 1. Note the more marked skin reaction in the lower portions of the exposed squares. They were covered by the elastic belt.

Variations in sensitiveness to the roentgen rays have been explained in many ways by various authors. Menstruation, pregnancy, diabetes, arteriosclerosis, gout, cachectic states, injuries, age of the patient, area of the body exposed, local anemia, dryness of the skin, acidity of the sweat, cutaneous vasomotor irritability and many other factors have been designated as the underlying causes of such variations. If it be assumed that skin reactions do vary, a careful elimination of some of the many controlling factors should in time lead to the determination of the actual causes of the so-called idiosyncrasy. Simpson[3] in 1916 advanced the theory that the severity of a roentgen ray dermatitis was dependent to some extent on skin temperature. He based his conclusions on the occurrence of severe reactions appearing after the applications of ice bags to the necks of his patients irradiated over the thyroid region. He was convinced that lowering the skin temperature caused an accentuation of the reaction observed.

The more recent work of Bovie and Klein[4] on unicellular organisms suggests that temperature changes may be of vital importance in roentgen-ray work. These investigators exposed Paramecia to rays from a small hydrogen discharge tube and found that the administration of a certain dose would produce no ill effects while the temperature of the culture was maintained at 17–18 C., but that most of the organisms died when the temperature was raised to 24–28 C. soon after irradiation.

FIG. 2. This animal has an adhesive band applied over the upper portion of the exposed area.

FIG. 3. The modified band used in applying a non-adhesive surface over the upper portion of the exposed area.

The work of Rohdenburg and Prime[5] on heat sensitization seems to point in the same direction. They observed that virulent mouse and rat tumors could be killed by exposure to a certain degree of heat or by a definite dose of roentgen rays.

However, the same result could be obtained by combining a sublethal degree of heat with a sublethal dose of roentgen rays. The heat could be administered either before or after the irradiation without changing the outcome. The temperature increase used in producing the combined effect was remarkably small. Kaestle[6] and James,[7] in articles published six years ago, claimed that an erythema could be provoked after a heavy treatment by hot applications or a hot bath.

FIG. 4. Rabbit R-3 one week after exposure and just prior to the removal of the protective band.

Such observations make it seem likely that the temperature of irradiated skin may be a factor in the production of a reaction. In support of this supposition we have often observed that treated areas lying in the axillae, in folds of the skin of obese patients, beneath pendulous breasts, and over the portions of the buttocks coming in contact with a chair, are likely to show a more severe reaction than areas which are more freely exposed to the air. This may possibly be due to the higher temperature maintained at these points although the effects of moisture, the reaction of the perspiration, the lack of air, etc., should be ruled out. A patient was treated some months ago who had been operated on for carcinoma of the rectum. An artificial anus had been made just above the original opening and it was necessary for him to wear an absorbent pad held in place by an elastic belt. This belt covered about half of each area treated over the lower abdomen. After several weeks had elapsed there was a definite tan under the belt, whereas the uncovered skin showed little reaction. A photograph of this patient is shown in Figure 1. Areas treated through heavy dressings will sometimes show very marked reactions. Areas covered by anointment by some well-meaning physician seem to show a more marked reaction than those to which some cooling lotion such as "whitewash" has been applied. Vigorous full-blooded individuals of goodly proportions are more likely to have marked skin reactions than are the pale, delicate, slender type. Hyperthyroid patients, who, according to our ideas of basal metabolism must be dissipating unusually large quantities of heat through the skin, often show a mild skin reaction a few hours after the treatment is administered. This type of

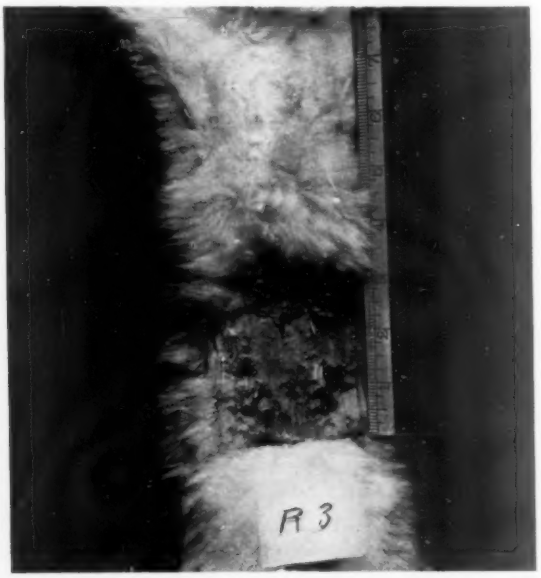

FIG. 5. Rabbit R-3 about one month after exposure. There appears to be a little more ulceration in the covered than in the uncovered portion of the exposed area.

early reaction is, so far as we know, rarely seen except in Graves' disease.

Since so much evidence seemed to point toward temperature as one of the governing factors controlling skin reactions, a series of experiments was undertaken in an effort to throw some light on the matter. Such work is time-consuming and this paper is presented more as a preliminary report than as a finished piece of research.

FIG. 6. Rabbit R-3 twenty-five days after exposure. The ulceration appears to lie mostly in the upper portion which bore a non-adhesive covering for eleven days after exposure.

The reaction obtained under the elastic belt made it seem probable that a simple covering might accentuate the skin effect. With this point in mind, shaved areas on the abdomens of rabbits were exposed to heavy doses of rays, after which they were partially covered with bands of adhesive plaster as shown in Figure 2. The areas were $3\frac{1}{2}''$ square and received 20 to 30 min. exposures with 5 ma. flowing and a $5''$ parallel gap at a skin-target distance of 10 inches. No filters were used. A broad focus Coolidge tube energized by an interrupterless transformer was used as a source of roentgen rays. The distal portion of each area was left uncovered in order that it might act as a control. After about ten days the adhesive plaster was removed. After applying this procedure to several animals, it became apparent that adhesive plaster applied over irradiated skin would pull away the superficial layers of the skin when it was removed, leaving a shallow ulceration at its site of application. This was objectionable since it brought in the additional factor of mechanical irritation. As a control experiment, a band of adhesive plaster was applied to a shaved area on the abdomen of one animal which had not been irradiated. The skin was not

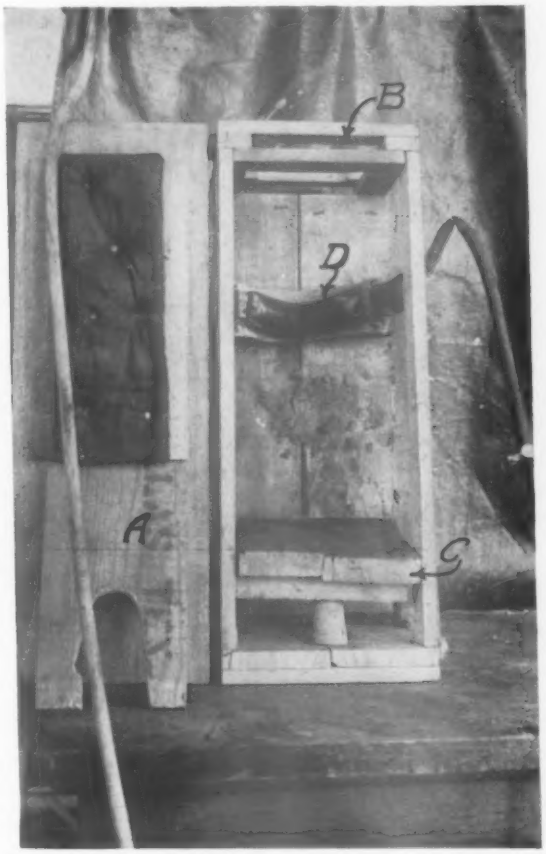

FIG. 7. The rabbit box propped up on end with the lid thrown open.

appreciably injured by the removal of the plaster at the end of ten days. These experiments seem to justify the warnings issued by certain roentgenologists against the application of adhesive plaster to irradiated surfaces.

A second set of experiments designed to eliminate the adhesive effect was next undertaken. To a band of adhesive plaster of the type used in the previous experiments, a second small strip was applied so that the adhesive surfaces were in contact

as shown in Figure 3. When the band was in place the non-adhesive surface covered the upper portion of the exposed area. The small strip was not quite as wide as the large one and this allowed a narrow strip of adhesive surface to lie across the middle of the exposed area. It was found necessary to do this in order to hold the covering in exactly the right position.

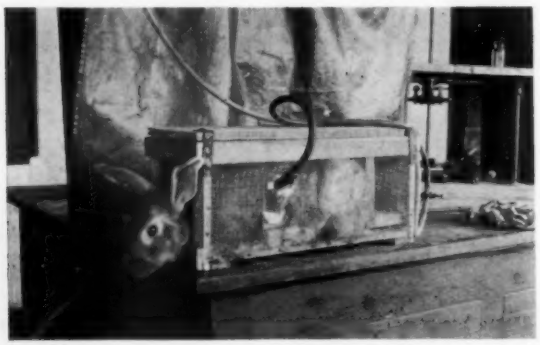

FIG. 8. The rabbit box in operation. The bag is held firmly against the animal's upper abdomen.

Rabbits R3 and R4 were exposed in the same manner as the animals used in the first experiment. The modified adhesive bands were then applied at once. Figure 4 shows rabbit R3 one week after exposure at the time that the plaster was removed. While taking away the covering a narrow strip of epithelium was pulled away with the adhesive portion, but no other variation from the normal was observed. At the end of the thirteenth day the entire exposed area showed a light even tanning and a definite contraction of the whole area parallel to the long axis of the rabbit. After a month had elapsed the appearance was that shown in Figure 5. The shallow ulceration which appeared at the point of application of the adhesive strip in the middle of the area slowly spread out in both directions, until it assumed the dimensions shown. The process is more extensive over the area that was covered than over the unprotected surface.

Rabbit R4 was treated in exactly the same manner except that the covering was allowed to remain in place for eleven days. Figure 6 shows the appearance of the lesion at the end of twenty-five days. It is evident that the greater part of the ulceration lies in the upper half of the area, that is, in the covered portion. For some reason it does not involve the whole of the exposed area from which the hair has been removed by the dose given.

These findings seem to indicate that a covering placed over an exposed area for a number of days after it is irradiated increases the degree of skin reaction obtained. It has not been definitely proven

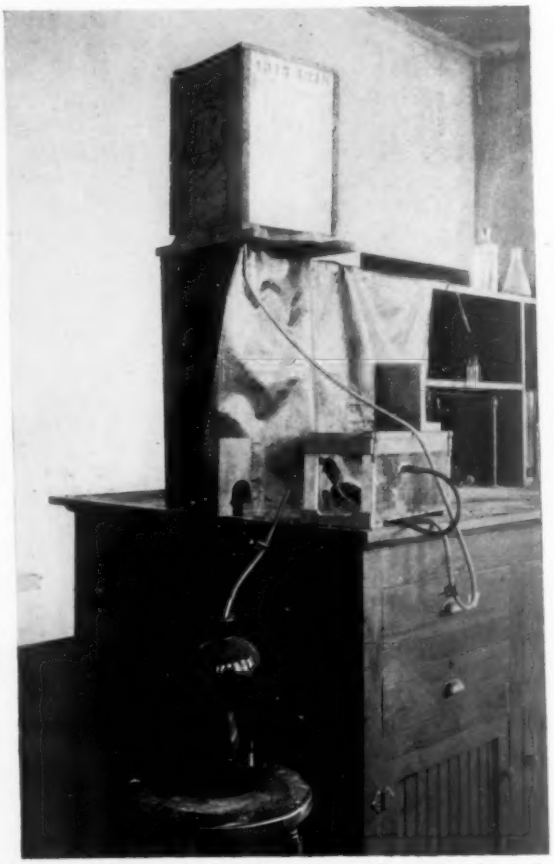

FIG. 9. View of complete apparatus showing the supply reservoir and the drain bottle below.

that the temperature beneath the adhesive strip was higher than that of the uncovered skin, but it seems likely that such was the case. Rabbits do not perspire, so that the influence of sweat is ruled out. The mechanical irritation set up by the removal of the narrow adhesive strip might possibly be the exciting cause of all of the ulceration produced. Perhaps future experimentation with more ideal types of covering materials will make the interpretation of these findings easier.

Attempts to study temperature effects

produced by merely applying a covering to the skin proved so unsatisfactory that the piece of apparatus shown in Figures 7, 8 and 9 was set up with the hope that the variable factors could be more accurately controlled. A rabbit box was so constructed that an animal placed inside could sit in a normal comfortable position, but could not move. This box is shown

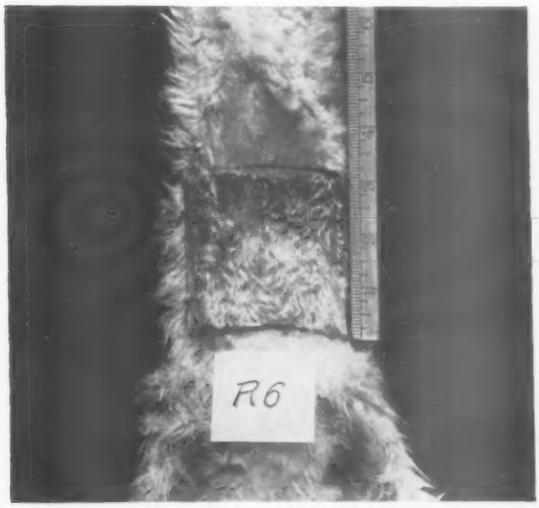

FIG. 10. Rabbit R-6 found dead thirteen days after exposure. Note the crusting over the upper portion of the area. It was this portion which was cooled.

FIG. 11. Rabbit R-8 twenty-four days after exposure. The crusting in the mid portion of the exposed area represents the location of the cooling bag.

standing on one end with the lid thrown open in Figure 7. "A" represents a slide which fits into the slot at "B." This slide has a hole in it which fits snugly about the rabbit's neck. "C" represents an adjustable back board which is used in making the length of the box suitable for animals of different sizes. The rubber bag marked "D" is set on an adjustable curved platform and is so arranged that it is pressed firmly against the abdomen of the experimental animal. Figure 8 shows the box set up with a rabbit in place. The box was devised for the purpose of cooling or warming a certain area of skin by allowing cold or hot water to pass through the rubber bag held in contact with the skin. Figure 9 illustrates the completed apparatus. The supply reservoir is placed well above the box, while the drain bottle is placed below it.

Since skin coverings seemed to accentuate the cutaneous reactions slightly, it was thought wise next to investigate the effect of cooling. Rabbit R6 was given a 30 min. exposure over an area from which the hair was not completely removed and then placed in the box so that the cooling bag only covered the proximal portion of the area. An attempt was made to cool the area for about 8 hours each day although this routine was not strictly adhered to with rabbit R6. The cooling was continued for a total of 33 hours distributed over 4 days' time. The rabbit was not healthy, and on the 13th day it died after showing a rapid loss of weight and developing a diarrhea, both of which symptoms pointed to a roentgen-ray toxemia. Figure 10 shows the exposed area as it appeared after the death of the animal. The tanning and crusting of the portion of the area covered by the bag is quite marked, whereas there is not even a loss of hair in the uncovered portion. This result was so striking and unexpected that a second rabbit, a large healthy individual (R8) was treated in the same manner. The cooling bag was adjusted so that it lay across the midportion of the area. This animal was in the cooling chamber during a total of 39 hours scattered over 5 days' time. It was put into a cage each night and given food and water. At the end of 8 days a definite erythema developed over the entire exposed area. At the end of 13 days the diameter of the area parallel to the long

axis of the rabbit had contracted until it measured only 2⅛ inches, and the whole area showed a marked tan and beginning desquamation. At the end of 3 weeks, sharply defined brown lines appeared within the tanned region. These lines lay parallel to the upper and lower borders and about ¼″ from them. It seems fair to assume that these lines represented the borders of the cooled area. At the end of 24 days the crusts that had formed between the brown lines and the outer borders of the area had practically all fallen away, leaving a smooth, hairless skin beneath. However, there was at this time a heavy crust over the cooled area. This crust was broken in many places and serum was seen oozing from beneath it. The appearance is illustrated in Figure 11.

These findings seem to substantiate the clinical observations made by Simpson and indicate that cooling the skin after roentgen irradiation is likely to accentuate the skin reaction. The element of moisture could not be eliminated in these experiments, since water was constantly condensing on the rubber cooling bag. In order that this work may be of definite value, many control experiments should be undertaken. As the weather becomes cooler it is our desire to try the effect of using warm water instead of ice water in the rubber bag and to determine the effects produced by various substances applied to the skin of the irradiated areas.

CONCLUSIONS

1. Adhesive plaster appears to produce ulcerations upon its removal from an irradiated skin surface.

2. The evidence at hand suggests that a covering placed over irradiated skin causes some increase in the reaction. This may or may not be due to an increase in the temperature of the skin.

3. Cooling the skin, by means of an ice bag for several days after irradiation, seems to accentuate the reaction obtained.

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DISCUSSION

DR. PARISEAU. The experiments of Dr. Martin gave a significance and support to an isolated fact that I observed a few months ago. I had occasion to treat a case of cervical adenitis and I decided to make two radiations. I decided to protect the lower part with lead, and in order to hold the lead in place I used strips of adhesive plaster. As the adhesive was of pure quality I heated it over a spirit lamp and applied it. I gave the first dose and the second. I had a very severe reaction, marking exactly the surface of the adhesive plaster. I first attributed it to the fact that I had burned my patient merely with a too highly heated plaster, but on observation I found that only that part which had received the roentgen rays gave severe reaction; therefore, I concluded that by using hot adhesive plaster applied to that skin I had hypersensitized it to the roentgen ray. This gives some support to the experiments of Dr. Martin who is right in saying that the study of temperature effects on skin sensitiveness is not new.

As far back as 1906 Dr. Hart of Paris gave a lecture in Montreal before the Medical Society and the object of his lecture was to bear out a law that he claimed to have discovered. All things being equal, the radiosensitiveness of tissues is the function of their temperature. However, Dr. Hart's law was not accepted then, because, I believe, it had insufficient experimental proofs. His work proved the hypersensitiveness of tissues when heated, but all his efforts to prove the hyposensitiveness of tissues were remarkably unsatisfactory. Therefore, the law could not be accepted because the extremes were not proved. There was part of his observation which today, in the light of our new work in deep therapy, may be of great significance. He claimed that heating the tissues made them hypersensitive to the *x*-rays and that he obtained better results in neoplasm of the uterus when he had previously, for several hours, given hot intrauterine douches.

However, I believe that Dr. Martin's work and such work on the sensitiveness of the skin is very precious, for we cannot speak, I believe, of a cancer dose. We must give as much rays as we can without harming the skin. Experiments which will show the causes of hypersensitiveness will help the progress of therapy.

Dr. G. W. Holmes. I would like to confirm some of the very excellent observations made by Dr. Martin. I have not the slightest doubt that the high temperature of the skin of goiter cases makes them unusually sensitive to radiation. In relation to adhesive plaster there is another factor to consider. Most plaster contains zinc, and, whether or not secondary radiation from the zinc has something to do with the producing of an erythema, I do not know. Not long ago, we rayed a patient who had adhesive strapping on the back. We rayed her for fibroid. She developed a well-marked erythema underneath the plaster only. The plaster contained zinc. After that experience, we used adhesive plaster that did not contain zinc, and placed it over small areas in cases being rayed over the breast. Erythemas were produced in these cases just the same (so far as I could tell) as in those cases where the adhesive plaster containing zinc was used. In other words, the increase in the erythema seemed to be due to heat, or rising of temperature, underneath the adhesive plaster, rather than to secondary radiation. Our experiments along these lines are not completed yet, and I do not wish to report them as accomplished facts. Ointments, as Dr. Martin mentioned, undoubtedly do the same thing. We have had considerable trouble with cases coming from the Skin Department, because there they put on ointments after treatment, with the result that we get a number of erythemas. With regard to cooling lotions, we have used "whitewash" for a long time, and, from clinical observations, we cannot help being convinced that it reduces the possibility of an erythema. In cases where we know we have given an overdose, we have applied whitewash immediately, and they have not developed erythemas. This has happened a sufficient number of times to be convincing. We had a case not long ago in which the filter slipped out during the exposure. I do not know how long it was out, but the dose through the filter should have produced a mild erythema. The case was a hospital patient and we applied ice bags and whitewash immediately and no erythema developed.

Some of the experimental work that has been done tends to show that any injury to a cell tends to increase the action of the rays, and it is possible that long-continued exposures to cold may injure them sufficiently to produce a bad effect. But cold applied for a shorter period of time may be beneficial.

Dr. R. W. Holmes. In connection with Dr. Martin's very interesting experiments and in line with Dr. Holmes' discussion, I want to report a rather trying experience I had in treating a case of hyperthyroidism. I treated the patient a number of times before (this was her fourth series) with no unusual effect. This time she came into the laboratory, was treated, and given the usual dose which I have been giving her, 9 inch spark gap, 5 ma., 10 inch distance, through 4 mm. aluminum and one thickness of sole leather for five minutes. I treated three areas. This was at 11 o'clock, and at 2 o'clock the nurse came to me and said the nervous patient was very much disturbed because of the redness over the areas treated. I found the portals of entry well defined on the skin with bright red erythema. That was four hours after the dose. In twenty-eight hours it had entirely disappeared and there was no more reaction. I have never been able to clear up in my mind just what it was that happened. There was plenty of space between the cone and the tissue treated.

Dr. Remer. We have made quite a few experiments in the use of drugs in connection with roentgenotherapy. We divide them into three classes—those which accentuate the action of the ray; those which are questionable; and those which are perfectly safe. We have found that there are between fifteen and twenty drugs which do accentuate the action of the rays. If the rays are to be used, these drugs should not be used ten days to two weeks previous nor for ten days to two weeks subsequently. Regarding the erythema which the last speaker mentioned, I think we frequently find that erythemas come on within three to twenty-four hours following the exposure and then disappear within twenty-four to forty-eight hours. We regard these as quite temporary erythemas and that they are ordinarily due to static electricity and can be overcome by grounding the lead.

Among the drugs which are contraindicated are iodine, stronger mercury preparations, salicylic acid, stronger solutions of nitrate of silver, balsam of Peru, and particularly scarlet red. Scarlet red will in every instance produce a third degree radiodermatitis where you would get only a mild erythema without its use. We have a case under treatment now in which scarlet red was used and we did not know it. The man was given sufficient radiation to produce a mild erythema and he afterwards developed a third degree burn which is still under treatment, and that is eleven months ago.

In the safe drugs we include carbolic acid, 2 to 4 per cent, zinc oxide, witch hazel, alcohol, bay-rum, boric acid, and plain vaseline.

The questionable drugs are stronger camphor preparations, picric acid, and menthol. The

reason we put picric acid among the questionable drugs is that we have had comparatively little experience with it.

Dr. Pirie. I wish to bring out this point that when a big dose is given to the skin and the skin reddens, the skin is then on the way towards destruction. The cells, as we have described before, are sickened and on the point of dying; if left alone they will recover. Take for instance a baby that is sick and on the point of dying. You are not going to apply strong ointments, extreme heat, extreme cold, irritating rubbing. All these things are harmful. It is the same way with the skin which has received a large dose of roentgen rays. It will react more strongly the more it is irritated, whether with cold, heat, with rubbing, or irritating ointments which you put on.

Dr. Pfahler. Major Simpson of the Fox Hills Sanatorium has claimed that the application of ultraviolet light will decrease the sensibility of the skin to radiation. I have never tried it, because I have been afraid to do so. I am wondering if anyone in this room has made any such experiments or can give information as to whether ultraviolet light will increase or decrease the sensibility of the skin.

Dr. Manges. It might be interesting during this discussion to tell of a little occurrence at Jefferson. A patient was treated for tuberculous glands of the neck. The filter had been taken out of the tube stand and the operator did not notice it. The patient got a full dose, 5 ma., 9 inch spark gap, 9 inch distance, five minutes. Of course, within two weeks reaction came on and was rather severe. Blistering occurred and the skin surface bled very freely. The only treatment the patient had was 5 per cent bicarbonate of soda ointment. I was astonished at the comparatively little discomfort, and also surprised at the rapidity with which the wound healed up. I am wondering whether the reaction from the hard penetrating rays was much less severe than the old roentgen-ray burns we used to get when we were using very much softer rays and therefore healing was very much more easily brought about. Nothing but 5 per cent bicarbonate of soda ointment was used and it healed up very promptly—in about six weeks.

Dr. Abbe. This is an extremely interesting paper—one that appeals to all of us. We are all interested in finding the causes which lead to these hypersensitive conditions. In connection with these experiments, perhaps in connection with the adhesive plaster, not only is the zinc one of the factors, but possibly also the non-zinc oxide adhesive; with the lead plaster—the lead is also one of the factors, it being sensitized and giving off secondary rays. I believe almost any of those heavy metals in ointments and in plasters are going to give the same effect. In connection with goiter cases, do we not have an increased iodine reaction, thereby stimulating metabolism, and giving an effect similar to what we get with iodine applications to the skin? Again, one of the features that was mentioned was menstruation. Is there not perhaps the same stimulation of metabolism going on in increased menstruation as there is in certain of these hyperthyroids? Do we not find that the cases with excessive menstruation or ovarian conditions, —not the bleeding of fibroids or intra-uterine bleeders, but the excessive menstruation which we see occasionally in a girl in her early teens, with the profuse everlasting flow that cannot be stopped, due to the metabolism proposition, —are not those cases sensitive to our rays, and is there not the same metabolism upset that we get in hyperthyroid cases? One thing in connection with dosage and static electricity. The first cases in which I noticed hypersensitiveness of the skin were hyperthyroid cases where applications of radium plaques were made. In them there was no question about dosage, except so far as time was concerned. The radium plaques were applied to the skin with merely a sheet of blotting paper as filter. In these cases frequently I found that an application of five minutes would produce a very definite transient erythema which was not produced in any other cases I have treated.

Dr. LeWald. I want to record an error in a milliampèremeter which led to an *x*-ray burn and a suit against the hospital, which is now pending.

I was asked to go to the hospital and express an opinion as to why this burn had occurred. There was no question but that the milliampère reading was fully 50 per cent too low, as evidenced by the appearance of the Coolidge tube.

Dr. Coolidge called attention to the possibility of a milliampèremeter going wrong in his remarks yesterday morning. I believe it is a thing we must always bear in mind. Dr. Coolidge recommends the use of two milliampèremeters or a careful checking of one meter from time to time.

Dr. Schmitz. We hardly ever treat a case in deep therapy that we do not produce an intentional burn, and it sometimes happens that the burn may be very severe. In treating these burns we have found that if we leave them entirely exposed to the air or on a sunny day expose them to the sunlight, and on a cloudy day use a therapeutic light, we get better results than by any other means. If we use external applications, we have found that

if vaseline is used the condition is made worse instead of better. In applying preparations with heavy metals, etc., which contain organic substances, we notice that no benefit is derived.

DR. FLYNN. I wish to cite a case that bears out Dr. Martin's paper very well. I had a patient, a very stout woman with rolls of fat over the entire abdomen. I used the usual distance of 10 inches, 9 inch spark gap, 5 ma., for six minutes. Instead of using a filter of three, two were taken out and one left in. She was given six minutes, and when she came back in a month she called my attention to the marked blistering between the rolls of fat. I believe this was due to heat or moisture between the layers of fat.

DR. VON POSWICK. I come from a town where there are many foreigners. When I went there we had quite a few roentgenray burns. My first case was very severe, and I thought a good deal as to what I should do. Now when anyone comes to me I will never treat the patient. The first thing I do is to get a paste made of zinc ointment, boric acid, and olive oil. Especially in acne cases I have the patient apply the paste over the entire face at night, and in the morning wipe it off with the olive oil. After this is done treatment is given for one week. By doing this I avoid the sensibility of roentgen rays.

DR. MARTIN (closing discussion). I want to express my deep appreciation of the ample discussion accorded such an incomplete piece of work. It has acted as a stimulus to further investigation.

In our adhesive plaster experiments, there was no effect from secondary radiation emitted by metallic elements in the plaster, since the adhesive strips were not applied until after the doses of roentgen rays had been given.

The application of ice bags to the skin, following an overdose of rays, with favorable results as described by Dr. Holmes, brings up a point of interest. A patient in our clinic received an overdose over the skin of the neck after a filter had slipped out of place. Ice bags were applied to the area for about eighteen hours. Only a mild superficial erythema resulted and the skin was healed in six weeks. Both our case and the one described by Dr. Holmes were treated with cooling applications over a period of time much shorter than that used in the animal experiments. This suggests that the time element may be a factor of importance.

Someone mentioned the electrostatic effect resulting from the use of a metal shield as the cause of an early erythema in hyperthyroid cases. It has been our custom to use the same shields in all our work and we have never observed the early erythema occurring on the day of treatment except on the skin of our hyperthyroid patients.

THE VALUE OF INTERSTITIAL RADIATION*

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DURING the past few years our knowledge of the physical and clinical actions of radium has been steadily increasing. With this, the problem of treatment in malignant diseases has become, largely, one of more accurate radium application.

At present, radium is undoubtedly the greatest single agent at our disposal in combating cancer. It does not, however, fulfil the first principle of being a cancer cure—that of being a constitutional agent. Work now under way with the intravenous use of an active deposit of radium may ultimately place it in this class. In the meantime, our main energy must be directed toward increasing its value for local application.

In the earlier use of radium, failure resulted in many fields, partly because of a lack of knowledge of filtration and dosage, but largely because it was a practical impossibility to place and retain the element in a position that would permit of adequate effect on malignant tissue without undue destruction of normal tissue.

In almost every instance, radium applied to the surface delivers the greatest intensity of its dose to the least vital part of the neoplasm. With even the simple skin lesions it is frequently necessary to produce

*Read at the Twenty-Second Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Washington, D. C., Sept. 27-30, 1921.

surface destruction in order to efficiently reach the deeper and more actively infiltrating zone of the new growth. Whenever it is applied to the surface of a lesion only a small fraction of its energy is directed toward the diseased part. The remainder is wasted on surrounding healthy parts—

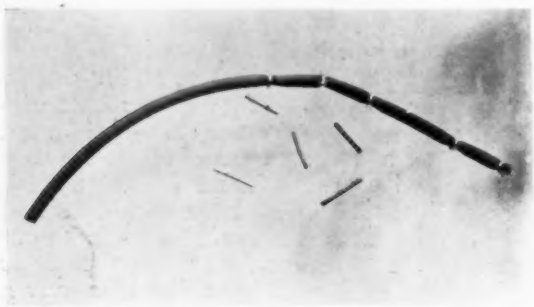

FIG. 1. Showing filtered tubes of radium arranged end to end in rubber tubing for insertion in base of operative wound.

frequently damaging them—or goes off into space. Furthermore, its practical application to a surface necessitates some sort of protecting filter which, in many instances, cuts off a large amount of useful radiation.

From both physical and economic standpoints it is therefore necessary to place the radium in such relation to the diseased part as to give uniform distribution and obviate loss of energy.

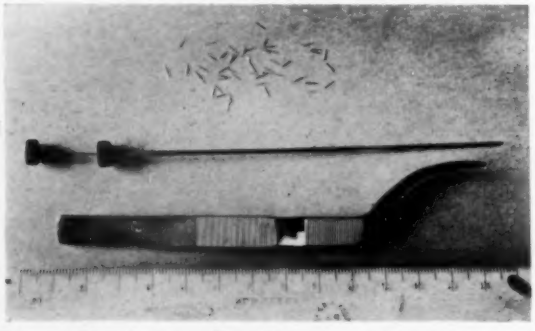

FIG. 2. Showing small radium emanation tubes and trocar needle used in inserting them interstitially.

That these principles have long been recognized is evidenced by the occasional reports of several years ago recording cases improved or cured by incising the tumor and laying in the base of the wound filtered tubes of radium. From a physical standpoint this was an advance, but the method could only be condemned because of the extensive and unwarranted surgical trauma. The Dominici tube obviated this question of trauma to a considerable extent and the principle still obtains for those who use the salt itself rather than the emanation.

FIG. 3. Section of rat's brain showing zone of necrosis about a 1 mc. emanation tube. (Courtesy of Dr. Halsey Bagg.)

The use of radium salt in small tubes, with a sharp point to facilitate introduction, buried within a tumor mass, is certainly a great advance over surface radiation alone. Trauma is a factor, but is much less than that incident to surgical exposure by incision of the mass. If sufficient tubes are available a proper and uniform distribution can be made. There are, however, certain limitations and drawbacks to their use. The size even of the smallest tubes prevents their use in some locations, especially if they have to be kept in place several hours. Even with the greatest care they may be dislodged by movement of the part or tension on the linen thread which acts as a guide and stay. The walls of the tube or needle cut out a very considerable amount of the beta radiation which is of definite thera-

peutic value. The period of radiation in most instances can be only for a few hours rather than for several days—a factor of importance if we believe that the malignant cell is most easily affected when in the process of division. The dosage must also be cut down to a smaller amount in view of the shorter period of radiation. Discomfort to the patient must be considered in lesions, especially within the mouth.

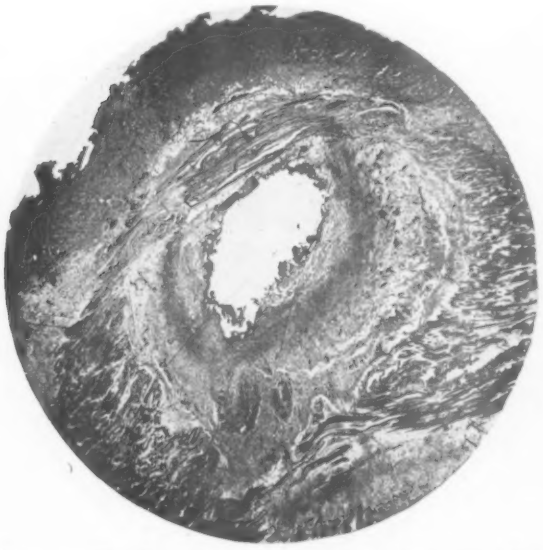

FIG. 4. Showing zone of necrosis and surrounding cellular infiltration about an emanation tube imbedded in rat tissue. (Courtesy of Dr. Halsey Bagg.)

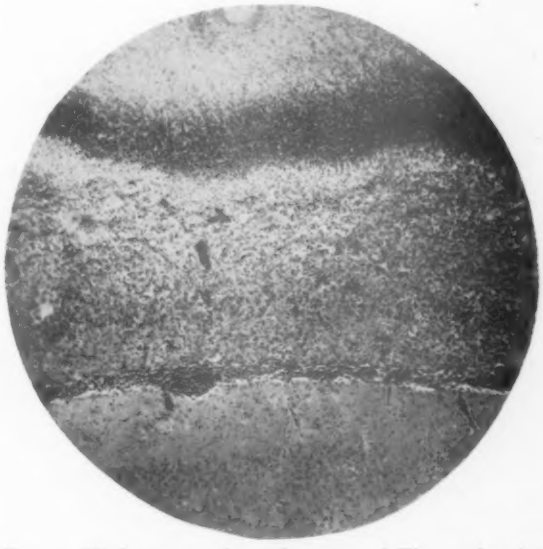

FIG. 5. High power view of a part of Fig. 4 showing zones of necrosis and lymphocytic infiltration.

The use of filtered radium in conjunction with surgery has a certain but limited usefulness. The placing of filtered radium tubes in rubber tubing at the base of the surgical field certainly places the element in closer proximity to the seat of disease and takes account of all of the available radiation emitted by the tubes, but it is questionable if this plan is the most efficient.

With the advent of therapy by means of radium emanation a new possibility was presented—that of burying small tubes of emanation interstitially and leaving them permanently in place.

At first the amounts of emanation in the tubes and the size of the tubes themselves led to severe reactions that made the otherwise good results seem of questionable value. However, with more clinical and experimental data we feel that we have now reached a practical working standard. Radium emanation is collected in fine glass capillary tubes 0.3 x 3 mm. in size, and in such concentration that each tube contains from 0.5 mc. to 2 mc. of emanation. For most types of work about 1 mc. per tube has proven to be the most suitable amount. In some very bulky tumors stronger tubes may be used, and, likewise, in very small lesions or in delicate locations, such as near large blood vessels or nerves, tubes of 0.5 mc. or less are most applicable. These tubes can be readily sterilized by boiling and Bagg[1] has shown that the emanation is sufficiently bacteriacidal to sterilize the inside of the tube. This makes their use safe, from a surgical viewpoint, even though a tube may be broken within the tissues. These small tubes are placed in the ends of hollow trocar needles, inserted within the tumor to the desired depth and then expelled by pressing in the trocar of the needle. Since radium emanation decreases in value at the rate of approximately 15 per cent per day, it will be seen that the total radiation to be derived from a given tube can be calculated, if its original strength is known. One mc. of radium emanation buried interstitially and left in place gives a total radiation equivalent

to 132 mc. hrs. It will also be seen that since the emanation decreases in value at only about 15 per cent per day a continuous radiation is kept up over a period of many days.

From the standpoint of trauma this method of introducing radium interstitially carries with it less danger than any of those previously mentioned. The tube is much smaller. The trocar needle is smaller than the needles containing the salt and is withdrawn as soon as the emanation tube is discharged, thus obviating extension of cells along its side. Even with this there is still a certain danger from trauma and it is suggested that in every instance where practicable a surface radiation with either radium or the roentgen ray be given first, similar to the manner of pre-operative radiation before surgical intervention.

The question of necrosis about these buried emanation tubes has been mentioned as a contra-indication to their use, but where properly used we have not seen untoward results. Bagg has shown that a tube containing 0.5 mc. of radium emanation produces a zone of necrosis 5 mm. wide around it and that a tube containing 5 mc. produces but very slightly more. This fact is of great importance. The beta radiation from a given tube extends only a certain distance, no matter whether a small or large amount is used. The gamma radiation is not so limited in its action. It is therefore possible to bury the tubes in such a manner as to care for considerable of the tumor tissue by beta radiation and the remainder by a heavy prolonged cross-fire of gamma radiation, without producing gross necrosis of the entire mass. This would be necessary if beta radiation were depended on entirely, and probably explains the failure to produce satisfactory and lasting results by the use of very weak tubes—0.1 mc. to 0.5 mc. The total amount of gamma radiation produced by them is not sufficient to do more than cause a temporary regression of all the malignant cells.

FIG. 6. Microphotograph of portion of a lymph node invaded by epidermoid carcinoma, eight weeks following the introduction of radium emanation tubes. Note necrosis of tumor cells and surrounding fibrosis.

FIG. 7. Low-power view of epidermoid carcinoma in lymph node four weeks after intensive external radiation. Note extreme fibrosis.

The tissue changes taking place outside the zone of necrosis about an emanation tube are both interesting and suggestive. The tumor cells show marked degenerative changes, irregular areas of necrosis, hypertrophy of cells and hydropic degeneration with hyperchromatism of cell nuclei. In addition, there is constantly a very marked lymphocytic infiltration. This suggests that the effect of radium is not entirely a destructive process directly, but that it stimulates to a marked degree the greatest natural factor in the body's defense of cancer invasion.

In introducing the tubes, care should always be taken to obtain as equal a distribution as possible and to keep well to the periphery of the growth. It is a grave error to keep most of the radiation toward the central part of the tumor rather than at the periphery which represents the actively growing infiltrating border. In

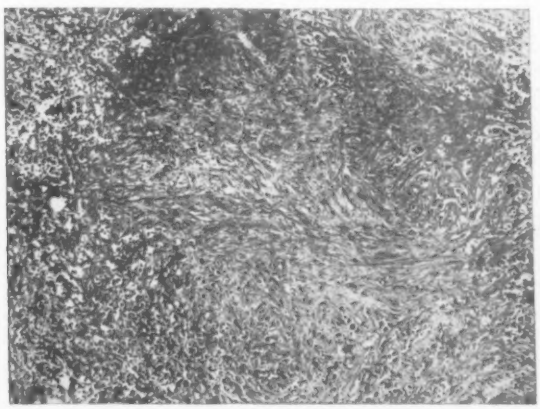

FIG. 8. Breast cancer eight weeks following interstitial radiation. Note destruction of tumor cells and fibrosis.

many instances it is well to implant emanation in the healthy tissue just at or as near as possible to the infiltrating base of the neoplasm. This can frequently be done by introducing the tubes through healthy tissue rather than through the growth itself. In fact, this avenue of approach is always preferable where practical. Tubes should never be introduced through an ulcerating infected surface unless drainage is free: they carry mixed infection deeper into the healthy tissues and add materially to the severity of the inflammatory reaction. Extensive sloughing may result from such a procedure.

As to the range of applicability of this method of radiation, it seems to the writer that our greatest use of radium in the future will be in this direction. While its use over the surface must not be underestimated, it is nevertheless a fact that the supply is limited, and except for use in certain inaccessible locations, such as the body cavities, must, for the great majority of cases, give way to some other form of radiation. Naturally we turn to the roentgen ray as the logical agent for this work.

Practically all new growths which present a distinct bulk of tissue can be managed best by the interstitial embedding of radium emanation. The shape of the neoplasm governs, to a certain extent, the dosage. A globular tumor requires less than an oblong or rectangular mass of equal bulk because of the greater cross-

FIG. 9. Rectal cancer two weeks after interstitial radiation. Note destruction of tumor cells and extreme lymphocytic infiltration.

fire. In the latter instance it is better to use more tubes of lower individual values in order to obtain a more even distribution without producing a reaction unnecessarily severe.

In certain locations it is almost impossible to keep surface applicators accurately in place and here again implantation of tubes obviates the difficulty.

In almost every group of malignant diseases the use of buried emanation has completely revolutionized the treatment. This is especially true of the intraoral work where surface applicators could be placed only in the anterior portion of the mouth, and even here could successfully care for only a small percentage of the cases because of the depth of neoplastic infiltration. At the Memorial Hospital we feel that the results to date have warranted us in excluding surgery entirely in the treatment of all primary intraoral lesions. A part of our results[2] published to date

deal with this phase of the work in detail. In the rectal, uterine, and breast groups, also, interstitial radiation has played a part that could not have been handled otherwise. A small but important group that has shown marked response to this form of treatment is the parotid tumors. A certain number of these, because of their susceptibility to radiation, respond satisfactorily to surface treatment while others because of their bulk cannot be adequately dealt with. Others are dangerous from a surgical standpoint because of their relation to the facial nerve. Embedded emanation has been most successful in these cases. Since the intense inflammatory zone about the tube is only about 1 cm. in diameter the nerve can be approached fairly closely without damaging its function or producing symptoms of nerve irritation.

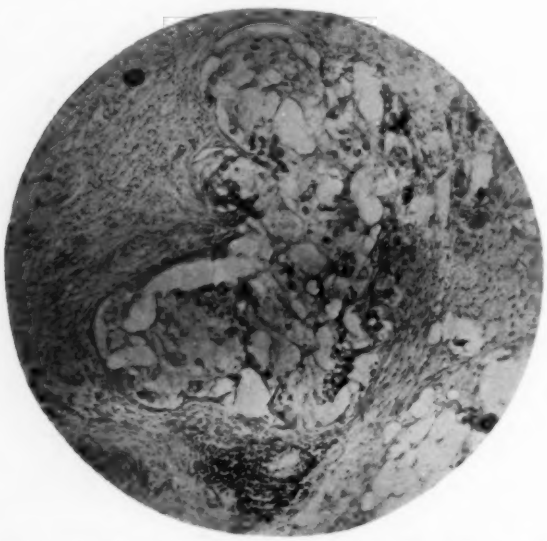

FIG. 10. Rectal cancer one month after interstitial radiation. Note necrosis and surrounding fibrosis.

In conjunction with surgery an added field of usefulness is opened for buried emanation. As previously reported[3] in detail, we use it as the method of radiation in all of our neck wounds following neck dissections. Many tumors are exposed surgically and are then found to be inoperable. Instead of closing the wound and resorting to external radiation, we are able to bury emanation accurately throughout the mass and close the wound without an added risk of suppuration. We now have a considerable series of cases of various types showing complete regression following this form of treatment and extending over periods of one to four years.

While comparatively little has been done as yet this combination of surgery and buried emanation holds important possibilities for dealing with many of the intra-abdominal new growths. With rectal cancer[4] we have found it a distinct advantage to approach some of the growths in this manner through the abdomen in conjunction with the usual treatment from below.

In connection with this subject of interstitial radiation the writer had hoped at this time to include a report of our work on the interstitial injection of an active deposit of radium but as yet the results are not sufficiently tabulated to publish.

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ROENTGEN RAYS OF SHORT WAVE-LENGTHS AND THEIR MEASUREMENT*

BY WILLIAM DUANE

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THE recent improvement in roentgen-ray therapy has emphasized the idea that we should use wave-lengths instead of voltages as estimates of penetration, etc. At its annual meeting in Cleveland, in 1914, I presented to this Society a method of measuring the average or "effective" wave-length of a roentgen-ray beam without employing a delicate and expensive *x*-ray spectrometer. We define the "effective" wave-length to be the wave-length of the monochromatic roentgen ray that has the same absorption as the whole beam of rays, in any particular case.[1] The method consisted in the measurement of the fraction of the radiation that passes through a given thickness of copper, or of aluminum. A curve giving the relation between this fraction and the "effective" wave-length determines the latter. The fraction of the radiation that passes through the filter may be measured either by a photographic or by an ionization method. The ionization chambers I was using at that time contained sometimes aluminum electrodes and sometimes plates of paper covered with a thin coating of graphite. The latter produces much less secondary and scattered radiation. In order to get an absolute measurement of the roentgen radiation in terms of the ionization current for a cubic centimeter of air, which served as a unit, I calibrated the ionization chambers actually used by means of one so constructed that the roentgen rays passing through it did not strike any solid body inside of it.

At the above-mentioned meeting and later before the American Physical Society I presented the curves representing the relation between the absorption and the wave-length. These curves show the wave-lengths corresponding to the amounts of radiation absorbed down to about .3 of an ångström. (We call the unit used for measuring wave-lengths the ångström.) It equals the one ten-millionth part of a millimeter.

2. In deep roentgen-ray therapy we use considerably shorter roentgen rays than .3 of an ångström, and the object of the research reported in this paper has been to extend the investigation of eight years ago to these shorter wave-lengths. Hull and Rice,[2] and Richtmeyer[3] have measured the absorption of roentgen rays by aluminum and copper down to a wave-length of .135 ångström. In the experiments I am now reporting we have measured the absorption by copper and aluminum of roentgen rays between .2 and .095 ångströms. This is the region of the spectrum used in modern deep roentgen-ray therapy.

3. Figure 1 represents the arrangement of the apparatus used in the measurements. The roentgen rays coming from the target, T, pass through a hole in a sheet of lead 1 cm. thick and in a brick wall and then through a narrow slit in a lead block, marked slit 1. The absorber or filter is placed between the tube and the wall, as indicated in the figure. After coming through slit 1 the roentgen rays strike a thin crystal plate mounted on a spectrometer table, by means of which its position and the angles it may be turned through can be measured accurately. The planes containing the molecules of the crystal reflect a part of the beam through a second slit (slit 2) into the ionization chamber. Slit 2 of the ionization chamber must, of course, be placed in the proper position to receive the reflected rays. The quadrant electrometer measures the ionization current flowing through the chamber.

In accordance with the well-known laws, discovered by Laue, Friedrich and Knipping, and by the Braggs, the crystal reflects

*Read at the Midwinter Meeting, Eastern Section of THE AMERICAN ROENTGEN RAY SOCIETY, Atlantic City, January 26–28, 1922.

only rays of certain definite wave-lengths, λ given by equation (1)

$$\lambda = 2d \sin \theta, \qquad (1)$$

where the angle, theta, is the angle between the roentgen-ray beam coming through the tube and the planes in the crystal containing its molecules. By measuring this angle, theta, we can determine from equation (1) the wave-length of the roentgen rays entering the ionization chamber.

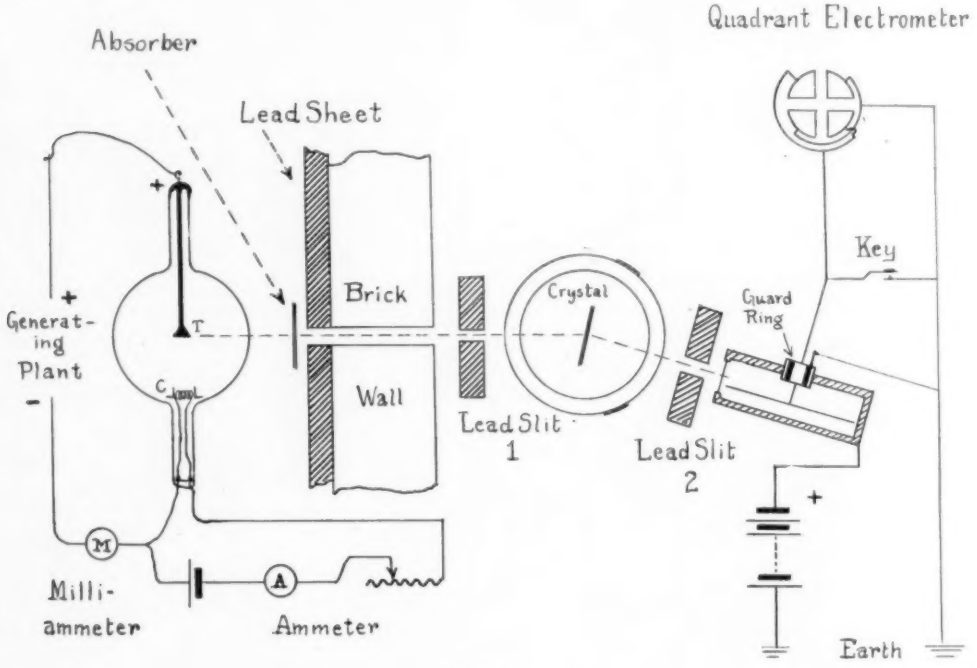

FIG. 1.

In making measurements with an *x*-ray spectrometer care must be taken to protect the instrument from stray radiation. It was for this purpose that the tube and generating plant were placed in one room and the spectrometer in another. In making measurements on the *absorption* of roentgen rays extreme care must be used in correcting for the natural ionization of the gas in the ionization chamber and for the ionization current due to any slight residual radiation. This correction has been made in all the experiments described below.

4. Before passing on to the methods of measuring the "effective" wave-length of a roentgen-ray beam, I shall give the results of experiments on the short wave-length spectra of roentgen rays that have passed through filters of 1 mm. of copper, and of 12 mm. of aluminum respectively. The curves of Figure 2 represent the data obtained in these experiments. The vertical coordinates give the ionization currents (expressed in arbitrary units) measured with the crystal in different positions, and the horizontal coordinates give the wave-lengths of the roentgen rays as calculated by equation (1) from the angles made by the beam of roentgen rays with the crystal planes.

It must be pointed out that the ionization currents represented by curves of this kind do not give the relative intensities of the roentgen rays at *different* wave-lengths, for various reasons, into which it is not necessary to go at this time. The ionization curves do give, however, the relative intensities of the roentgen-rays of the same wave-length in different cases.

Curve A in Figure 2 represents the *x*-ray spectrum with 1 mm. of copper as a filter, and Curve B, the spectrum with 12 mm. of aluminum.

In these experiments the current through the tube amounted to 1 ma. The voltage

applied to the tube was constant and equal to 161,000 volts.

It appears from the figure that both ionization currents begin at wave-length .0755. This is the short wave-length limit of the spectrum determined by the voltage, v, applied to the tube,[4] in accordance with equation

$$v\lambda = 12{,}350 \qquad (2)$$

Both curves show maxima of ionization currents: Curve A for copper at wave-length .120 and curve B for aluminum at

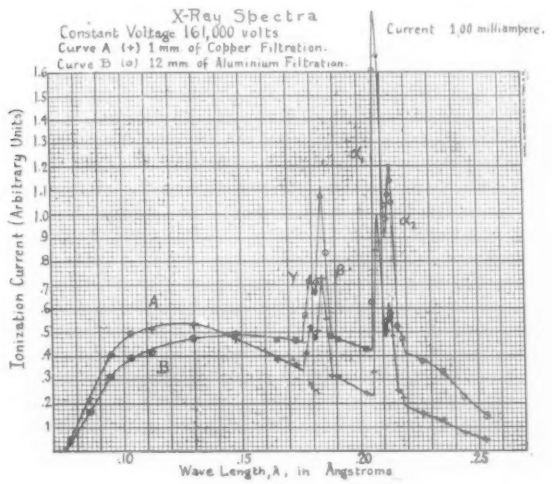

FIG. 2.

wave-length .145, the maximum for copper being slightly greater than that for aluminum.

At wave-length .141 the curves cross each other. For roentgen rays of this wave-length 1 mm. of copper absorbs exactly as much as 12 mm. of aluminum. For wave-lengths shorter than .141 the 12 mm. of aluminum absorb more radiation than the 1 mm. of copper; whereas for wave-lengths longer than .141 the 1 mm. of copper absorbs more radiation than the 12 mm. of aluminum. In other words, the relative filtering power of copper and aluminum is different for roentgen rays of different wave-lengths.

Since the aluminum lets through a much greater amount of long wave-length radiation than the copper and a much smaller amount of short wave-length radiation, copper is a much better substance to use as a filter than aluminum is, if we wish to produce a beam of roentgen rays of short wave-length. The effect appears to be very marked; for at wave-length .1 of an angström 30 per cent more radiation gets through the 1 mm. of copper than through the 12 mm. of aluminum, whereas at wave-length .2 of an ångström about 1.7 times as much radiation gets through the 12 mm. of aluminum as through the 1 mm. of copper.

The tall peaks on the curves represent the x-ray spectrum of the Tungsten target in the x-ray tube. The chief peaks (at wave-lengths .1790, .1842, .2088, .2134) correspond to the four principal lines in the Tungsten K series of roentgen rays.

Although these peaks are high, this does not mean necessarily that a large total amount of energy is radiated at these wave-lengths; for they are narrow, and it is the area underneath the curve in any region that corresponds to the intensity of radiation, rather than the height of the curve.

The radiation becomes practically zero in the neighborhood of wave-length .26, and we may regard this as the long wave-length limit of the x-ray spectrum for the filtration used.

5. It appears from the curves of Figure 2 that the roentgen-ray beam consists of a great variety of wave-lengths (from .0755 to .26). The fact that an x-ray tube does not produce a beam of roentgen rays of a single wave-length (monochromatic beam), even although the voltage applied to it may be absolutely constant (a storage battery, for instance), was pointed out in the article, published in 1915, above referred to.[4] There is, however, a certain average or "effective" wave-length, which has the same coefficient of absorption as that of the whole beam. It is this "effective" wave-length which is important to determine in many cases—for the physiological effect of the rays appears to depend upon the amount of radiation *absorbed*.

I shall now indicate how the "effective" wave-length of a roentgen-ray beam can be approximately estimated without the use of an x-ray spectrometer. The curves in Figure 3 represent the fraction of the radiation at different wave-lengths that passes through a given thickness of metal.

Curve A is for 1 mm. of copper and curve B is for 4 mm. of aluminum. These curves have been drawn from data obtained by actual measurements. If we determine in any particular case, the fraction of a roentgen-ray beam that passes through 1 mm. of copper, we can read off directly from the curve the wave-length of the monochromatic roentgen ray which is absorbed to the same extent by copper, and this wave-length is the "effective" wave-length of the whole beam.

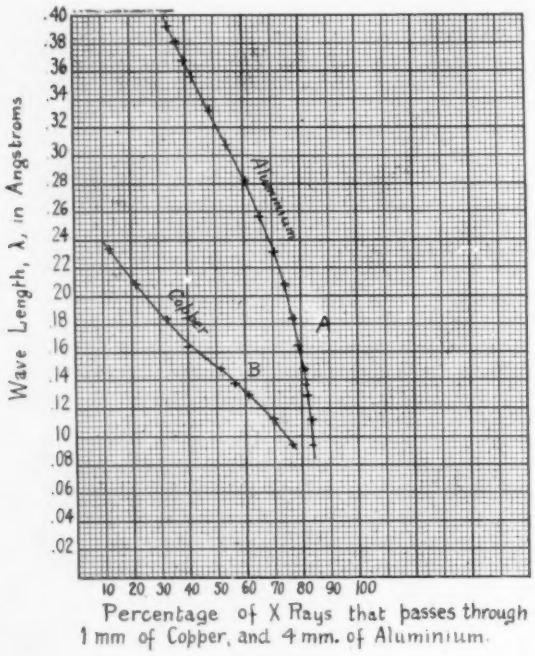

FIG. 3.

The same process may be followed with a filter of aluminum. If the filter is 4 mm. thick, curve B will give us the "effective" wave length of the roentgen-ray beam.

The fraction of the roentgen radiation that gets through the filter in any particular case may be determined either by means of an ionization chamber, or by means of photographs. In the case of the photographic method a rough estimate may be made by determining the lengths of time required to darken two portions of a photographic plate to the same extent where one portion has been exposed to the beam of roentgen ray without the metal plate and the other portion with it. The shorter of the two lengths of time divided by the longer gives roughly the fraction of radiation passing through the plate.

A few examples of "effective" wave-lengths in particular cases may be of interest. If the voltage applied to the tube amounts to 80 or 90 thousand volts, the "effective" wave-length generally lies between .20 and .29, depending upon the filtration used and the character of the generating plant. It will be noticed that these "effective" wave-lengths average somewhat longer than the wave-lengths of the lines of the Tungsten spectrum. If the voltage amounts to 200,000 volts and is produced by an alternating generator, the roentgen ray beam that gets through a filter of .70 mm. of copper has an effective wave-length between .14 and .15 of an angström.† A roentgen ray beam of about the same effective wave-length can be produced by a *constant* voltage of 165,000 volts.

It appears from the curves in Figure 3 that a more rapid change in the fraction of rays that gets through the 4 mm. of aluminum occurs at longer wave-lengths than at shorter wave-lengths. In the case of the 1 mm. of copper, however, a rapid change occurs at shorter wave-lengths. Hence a more accurate estimate of a short "effective" wave-length can be made by using the 1 mm. of copper than by using the 4 mm. of aluminum. For longer wave-lengths, the aluminum should be used.

6. In the method of measuring effective wave-lengths described in Section 5, it is necessary to have some means of measuring the relative intensity of two beams of roentgen rays. I shall now describe a method of estimating "effective" wave-lengths by comparing two beams of roentgen rays, and determining under what conditions they produced *equal* effects. These equal effects may be photographic, ionization or fluorescent.

In Section 4 I called attention to the fact that the relative absorption of roentgen rays by copper and aluminum differed for different wave-lengths. In other words, the thickness of aluminum that has the same filtering power as that of 1 mm. of copper

† I am indebted to Dr. Wilhelm Stenström for the tests from which this estimate has been made.

depends upon the wave-length of the roentgen ray. For long roentgen rays the thickness of aluminum must be large, and for short roentgen rays, small. The curve in Figure 4 gives the thickness of aluminum that is equivalent in absorbing power to that of 1 mm. of copper for a variety of wave-lengths ranging from .095 to .40. It appears from this curve that for the long

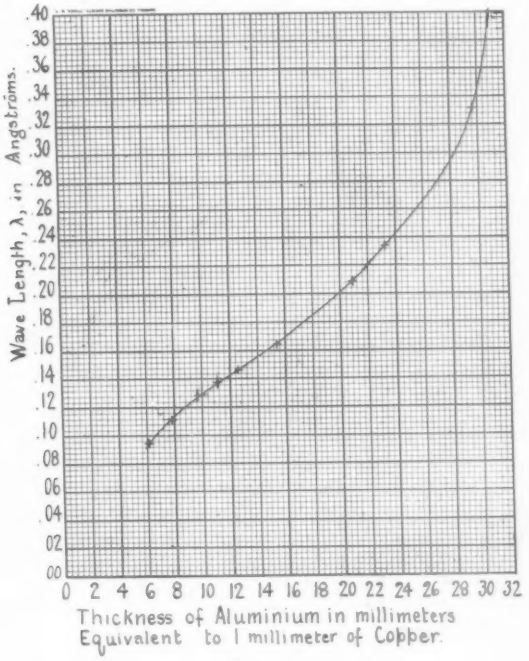

FIG. 4.

wave-lengths it takes about 31 mm. of aluminum to absorb as much as 1 mm. of copper, whereas, for short wave-lengths (about .1 ångström) 6 mm. or so of aluminum absorb as much roentgen radiation as 1 mm. of copper. If we determine by means of an ionization chamber, or by means of a photographic plate or a fluorescent screen, the thickness of aluminum that absorbs the same amount of radiation as 1 mm. of copper, we can determine from the curve of Figure 4 the "effective" wave-length of the beam of roentgen rays, —that is, the wave-length of the monochromatic roentgen-ray beam that has the same relative absorption in copper and aluminum as the whole beam.

It will be noticed that this method of estimating the "effective" wave-length resembles somewhat the use of a Benoist penetrometer. The fundamental principles of the two methods, however, are quite different from each other. The Benoist penetrometer depends upon the fact that at a certain wave-length (.485) a sharp increase in the absorption by silver occurs. In the present case there is a continuous change in the relative absorption of copper and aluminum with the wave-length. Further, in the case of the Benoist penetrometer the beam of ray for which the greatest thickness of aluminum is equivalent to the silver has the most penetration. In our method, however, the short wave-lengths correspond to the smaller thicknesses of aluminum that are equivalent to the copper.

The effective wave-length of a beam of roentgen rays decreases as it passes through the plates. In fact, the fundamental principle of filtration is based on such a decrease. Hence in estimating effective wave-lengths by this method, plates of aluminum and copper that are not too thick should be employed. For instance, in the case of short waves, we should use perhaps ½ mm. of copper, and determine the thickness of aluminum equivalent to it; whereas, for longer waves (produced by 80 to 100,000 volts) we should use ¼ mm. of copper or less, and determine the thickness of aluminum equivalent to it. Substances other than aluminum and copper may be used in the measurement. The data for some of these will be published in another paper.

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THE AUTO-ELECTRONIC DISCHARGE AND ITS APPLICATION TO THE CONSTRUCTION OF A NEW FORM OF X-RAY TUBE*

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The tubes at present in general use depend for the production of the electrons upon one of two effects, known for many decades: Either the ionization of residual gases (gas-filled tubes) or the heating of the cathode to a bright glow (thermionic or Edison-Richardson effect). The tube to be described, however, utilizes the electrons released by a phenomenon which I have only recently discovered. The main feature of the phenomenon is the releasing of the electrons on an unheated and cold cathode in the absence of any kind of ionization, either the ionization of the residual gases or an ionization through a hot spark discharge (electric arc) which pulls molecules from the electrodes and converts them into ions.

1. According to the facts observed, it is at least in a primary way the electrostatic field itself—hence the name "auto-electronic"—which liberates the electrons on the cathode.

For a clear understanding of the principle upon which this new tube is constructed, it will be necessary to first discuss certain physical experimental phenomena which have a bearing on this method of the liberation of the electrons.

CONDITIONS ESSENTIAL FOR THE PRODUCTION OF THE AUTO-ELECTRONIC PHENOMENON

The conditions which must be fulfilled, in order to realize the auto-electric discharge are:

2(*a*). The electrodes are to be sufficiently close together, up to 10 mm. or a little more. The voltage required by a certain milliamperage increases with the distance between the electrodes.

3(*b*). The cathode is to be provided with "active surfaces" having a small radius of curvature. These may be edges or points. Under identical conditions the voltage increases with this radius. It also increases if metallic surfaces connected to the cathode and situated close to the active cathodal point are introduced. This is evidently due to the deflection of the lines of electric force from the cathodal point produced by such accessory surfaces. This deflection is also the reason why when two points of identical radius of curvature, but of different angles, are compared, the more obtuse angle requires the higher voltage.

The conditions outlined in paragraphs 2 and 3 are of a geometrical nature, and speaking physically simply mean that to start the discharge a certain intensity of the electric field on the active surface is necessary. Besides these conditions there are also others of a non-geometrical nature which are essential for the production of this phenomenon. These are as follows:

4(*c*). A very high vacuum—higher, indeed than in the usual hot cathode tube—must be maintained. To this end an additional arrangement has been provided (Par. 20). A slight increase of the pressure beyond a certain value may cut the milliamperage, corresponding to a given voltage, down to 50 per cent of its proper amount, or even more.

5(*d*). Highly refractory metals, such as Wo, Ta, Mo and others, must be employed for the electrodes, because—in order to fulfill the conditions above—the electrodes, first of all the cathodes, must be heated by an electronic bombardment as high as possible during the process of evacuation. Gas absorbed at the cathodic active surfaces may interfere with the releasing of electrons and reduce the effect greatly. If such troublesome gas rests occur during

*Monatsb. der Saechs. Akad. d. Wissensch. Math.-Phys. Klasse, Leipz., Sitzung von 19 Juli, 1920, lxxii, 31. Verhandl. d. Deutsch Phys. Gesellsch., Sitzung von 25 Feb., 1920, s. 13.

*Read at the Twenty-Second Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Washington, D. C., Sept. 27-30, 1921.

exhaustion, then the regular discharge can be restored by energizing the tube with much higher tension, or by glowing the cathode intensely, or by a combination of both means. With a further increase of the gas density up to a certain limit, a further increase of the voltage is required to start the discharge. Beyond this limit, no pure electronic discharge is produced, a regular hot spark or electric arc being the only phenomenon that can be called into life by an increase of the voltage. Therefore, if evacuation is not cautiously carried out, either no discharge at all, or a regular gas discharge, or most frequently a hot spark (arc) discharge takes place between the adjacent parts of the electrodes. On the other hand, if the active metallic surfaces contain much more gas, or if the vacuum is much lower, a hot spark or arc discharge may be started with a much lower voltage. In the case of such ionic discharge, the cathode need not necessarily be pointed. The arc discharge may sometimes cease and leave a local ionization, and this may produce a local regular gas discharge, delivering *x*-rays; which, however, has nothing in common with the geometrically defined phenomenon referred to in paragraphs 2 and 3. On the contrary, the latter conditions have delayed the discovery of the auto-electronic phenomenon until now, because it was believed that there would be rather a tearing off of molecules from the metal and a starting of a hot spark discharge between the electrodes, before any release of electrons from cold cathodes by action of the electric field alone would take place.

6(*e*). The pure phenomenon is greatly facilitated by making the active parts of the anode hollow. This apparently handicaps the formation of an arc discharge and so facilitates the exhaustion, because an arc is capable of deforming the electrodes in a short time. A plane anode is less useful for the purpose and a convex is still less satisfactory.

PHYSICAL FEATURES OF THE AUTO-ELECTRONIC PHENOMENON

The characteristic properties of the phenomenon are:

7(*a*). The discharge is started by loading the active cathodic surface negatively only, so that the tubes have the character of valves.

8(*b*). The cathode is not heated by the discharge. Changing the temperature of the cathode within wide limits does not affect the phenomenon. In one of the initial experimental tubes the cathode was made hollow, so that it could be filled with liquid air. This cooling produced no effect but a slight increase in the milliamperage by some 5 to 10 per cent. which may be attributed to the fact that such cooling condenses some of the residual gases, and so improves the vacuum.

9(*c*). No change whatever is to be observed on the cathodes even by energizing the tube for several hundred hours (8). Consequently, the voltage required for a certain milliamperage is simply determined by the form given initially to the cathode (3), providing the exhaustion is sufficiently high and has been so carried out that no hot spark or arc discharge has taken place, which would result in deforming the active part of the cathode, while the tube is on the pump and during the period when the gas has not yet been sufficiently removed.

10(*d*). I have not yet been in the position to determine with accuracy the characteristic of the discharge (21). The voltage and the penetration of the *x*-rays increase definitely but not considerably with the milliamperage, to some 5 ma. With heavier current the increase of voltage, with rising milliamperage, appears to be very much slower. Correspondingly the penetration of the roentgen-radiation increases in this range very slowly with the milliamperage, so that the function characteristic of the discharge seems to contain a higher power of the voltage; probably it will prove to be an exponential function (21).

11(*e*). As far as superficial experience shows, there is a difference between different metals used as cathodes. However, the precise research of this question is still to be made.

12(*f*). The electrons are not necessarily driven from the cathode to the next adjacent point of the anode, but sometimes travel along a path of many centimeters to more distant spots of the anode, if a

suitable construction of the electrodes is provided; *e.g.*, if a pointed cathode (K, Fig. 1) is placed opposite a wire (A) being the anode, the point of impact of the cathode rays may be found at any point along the wire; and if a disc (C) is provided in the thicker rear part (B) of the anode, occasionally the cathode beam strikes it, heating it to a bright glow. The velocity of the electrons is evidently accelerated to a very high degree, very close to the active part of the cathode—which amounts to the statement that a very large increase

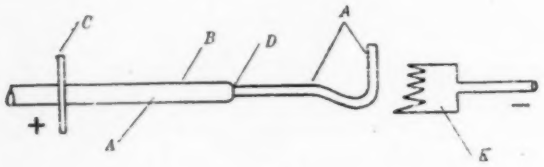

Fig. 1.

of potential takes place exceedingly close to the active cathodal surface. The residual electric force in the remaining space is of itself no longer sufficiently intense to direct the cathode beam towards the adjacent parts of the anode, once the electrons have been accelerated on the cathode.

13(*g*). Looking at the facts superficially, the sudden acceleration of the electrons along an extremely short path—practically on the surface of the cathode itself—may, from the standpoint of the theory of electromagnetism, be held responsible for a further new phenomenon which is the emission of a roentgen-radiation from the active surfaces of the cathode, while the other parts of the cathode do not emit any radiation whatever. This is proven by pinhole camera pictures of energized tubes exposed twenty times or more longer than would be required to obtain a picture of the anode. The radiation from the cathode is a weak one and specifically negligible if compared with that of the anode. Furthermore, the radiation is much softer. If a tube is operated with a 15 cm. spark gap, the regular radiation of the anode is but slightly affected by a two millimeter aluminum filter. The cathodal radiation, however, is completely absorbed by such a filter. Through so thin an aluminum layer no impression of the cathode radiation whatever can be obtained on the photographic plate.

A pinhole camera picture of a tube, consisting of a plane Pt-Tr target and a crown-shaped cathode is reproduced in Figure 2. The cathode is made of a tantalum wire of 6 mm. in diameter, drilled out into the shape of a thin-walled cylinder and cut out zigzag, making seven V-shaped points, of which the inner cutting edges are kept sharp. On the anode appears a number of focal points, one of which appears in the picture. Of the cathode the

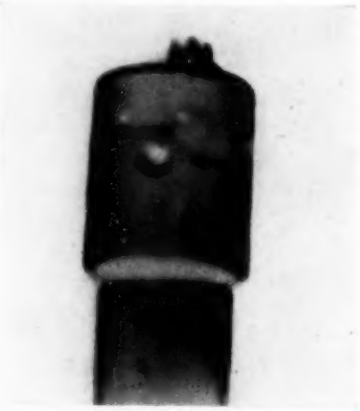

Fig. 2.

edges only can be seen. The tube was operated on a rectified direct current. A reliable highly evacuated hot cathode valve and a high tension resistance of 10 megohms of carbon-coated glass tubes was inserted in series with the tube studied, in order to exclude any inverse voltage which might have possibly arisen from some electric oscillation. The picture has been taken by exposing a glass plate, provided with an intensifying screen, to a radiation generated by 2 ma., 180 minutes at a focal distance of 25 cm., through a pinhole of 0.8 mm. The distance of the pinhole to the plate was 10 cm.

14(*b*). Another fact appearing to be closely connected to those under paragraphs 11 and 12 concerns the ponderomotive force of attraction between the electrodes. One of the electrodes—for instance the cathode—may be held at a certain distance from the anode by a spring of such dimensions that the regular electrostatic attraction does not visibly reduce

the gap between the electrodes. If the spring is not too resistant, there is always a considerable pull on the cathode to be observed as soon as a discharge of about 3 ma. is started. The pull increases with the milliamperage. Another example is that of several springs mounted on the anode; such springs are apparently subject to attractive force only if they are struck by the cathode beam. As long as the cathode beam does not touch a particular spring, no expansion is to be observed, although a heavy discharge may be kept going to a different part of the same anode. This effect of anomalous attractive force has also been studied by means of an apparatus, in which the cathode was mounted upon one arm of a balance, the other arm bearing an iron core, which was under the influence of a magnetic field that could be varied from outside the tube. I have been very much interested in the measurements of the anomalous attractive force, because I feel that an exact knowledge of this effect may become a basis for determining important features of the nature of the electrons, and, broadly speaking, of electricity and matter.

THE X-RAY TUBE

15(*a*). It is obvious that the new effect may be applied for realizing any technical application of a flow of electrons. Either the electrons given off primarily by the cathode, or the secondary flow of electrons coming from the spots of impact of the primary rays on the anode, or both, may be used for any electronic device as an *x*-ray tube, an amplifier, etc.

In the present paper the application to *x*-rays only will be described. Naturally any cathode provided with active surface of a small radius of curvature and adjacent to an anode of any kind will be capable of producing *x*-rays if only the dimensions are chosen so as to make the discharge pass at the voltage required for the particular purpose. Nevertheless several constructive features must be introduced in developing the simple fundamental idea into a satisfactory practical device.

16(*b*). First of all, the shape of the cathode is to be chosen. Simple points, straight and circular edges, multiple points, mounted like points of a crown, have been tried. When a heavy milliamperage would be required—especially with low voltages—multiple points will have to be used. For regular *x*-ray work, however, simple points appear to act satisfactorily, since the milliamperage increases very rapidly with the voltage (10). The points must be grounded with greatest care and precision (since the angle and their sharpness decides the hardness of the rays) on wires of a diameter of two or three millimeters. The wires are of Wo, Ta, Mo or other refractory metals,

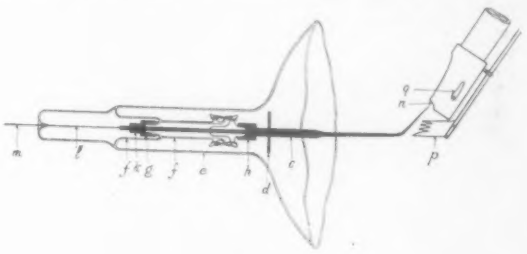

FIG. 3.

because the cathodes must be glowed out, up to a bright white glow during the process of exhaustion. The cathodic wires are mounted on holders (*c*, Fig. 3) which may consist of one of the metals mentioned or even of a less refractive material, like iron, etc. The small disc (*d*) of metal overshadows the insulating parts supporting the shaft of the cathode, in order to prevent electrons used during the process of exhaustion for the bombardment from impinging thereupon. The parts fixing the cathode properly are arranged in the neck (*e*) of the bulb. They are very elaborately designed and constructed, since an absolute fixation of the cathodic point is required to secure a constant working of the tube. Inside of the neck, another glass tube (*f*) is sealed, both ends of which act as bearings for the shaft of the cathode. The cap (*g*) of the rear part of the shaft fits tightly upon the glass tube, while the frontal cap (*h*) is tightly mounted on a quartz support (*i*), the last being fastened by means of pieces of flexible wire to the front end of the glass tube. The threaded part (*k*) of the shaft enables the glass blower to fix the cathode holder exactly upon the tube. The connection to the cathode is obtained by a flexible wire, whose outer end (*m*)

is sealed into the glass. A photograph of two points of different sharpness is represented by Figure 4.

17(*c*). The anode of the tube may be designed for any system of cooling. That is, of course, no characteristic feature of the tube. The tubes demonstrated in this country are equipped with anodes of the water-cooled type. For the target any of the known materials may be used. The only important feature of the anode is that

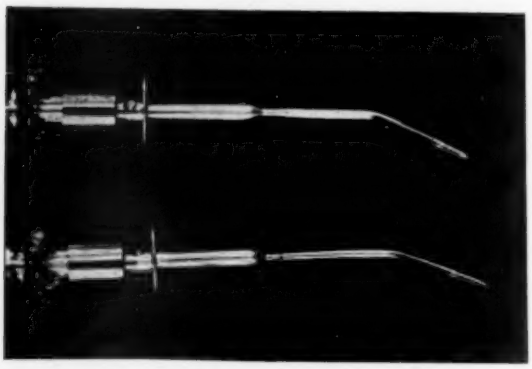

FIG. 4.

the target is hollow over the area, (*n*, Fig. 3), where the focal spot appears. There are two reasons for this:

(I). First, if a convex or plane target is used, the discharge has a tendency to form one or more extremely small focal spots, so that the load per unit of target's area may become too heavy even with a medium milliamperage, which would result in melting or evaporating the metal. On the contrary, if a cavity on the target is used, the sharp pencils of cathode rays are distorted by the particular shape of the electric field. With small milliamperage the focus consists of a few spots—when increasing the milliamperage, the spots become larger and more numerous, so that the area struck by the primary electrons enlarges with the current (Figs. 2 and 3), a very favorable feature. The dimensions of the anodic cavity have to be adapted to voltage and purpose. For lower voltages such as used for roentgenographic and fluoroscopic purposes, a cavity of 4 mm. depth by 5 mm. diameter appears to be sufficient. Inasmuch as there is not only impact of electrons on the points struck by the primary cathodic beam, but also on account of a great number of secondary electrons started in these points, the entire cavity acts as a focus, emitting *x*-rays (Fig. 5, 1). Therefore, the dimensions of the cavity are of importance, in so far as the sharpness (definition) of the pictures is concerned. The dimensions mentioned have proven—as experience shows—to be completely satisfactory.

To avoid the shadow cast by the cathodic point, the focal cavity (*n*, Fig. 3) is located not in the center but close to the

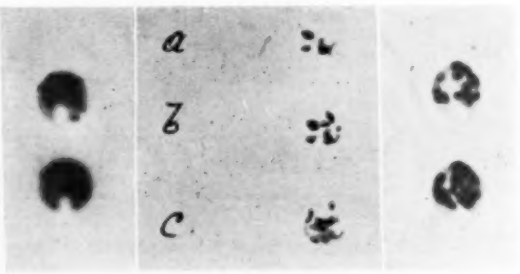

FIG. 5.

edge of the anode, so that the cavity is cut by the edge.

A cavity of much larger dimensions must be provided if the tube is to be energized by high voltages. For use in deep treatment, a diameter of 14 mm. and a depth of 16 mm. or even more may be chosen. This answers not only the purpose of distorting the initial small diameter of a more accelerated (on account of the higher voltage) cathode beam, but has the additional virtue that such a cavity may act simultaneously as a protective shield if it is designed so as to limit the intense primary *x*-ray beam to an angle not much larger than that required for the particular purpose.

(II). It is well-known that the hot cathode tubes emit a very considerable portion of radiation outside of the intended focal spot—from the rest of the frontal part of the target and to quite a large extent even from the stem of the anode. This radiation is due partly to a necessarily defective focusing of the cathode beam, partly to secondary electrons which are started by the primary ones at the focal point and bent inside of its double layer close to and in front of the focal point,[1] so as to strike

[1]Jahrb. f. Radioaktivität u. Elektronik, xvi, 105–189, especially 140–159.

upon the target and the stem This "stem" radiation is obnoxious, not only because it fogs the image on the sensitive recording surface, but also because its intensity is lost for the useful purpose and must, therefore, be deducted from the total radiation. By making the anode hollow the amount of this "stem" radiation is reduced to several per cent of its usual value, and practically can be neglected. It is evident that any tendency for the electron to

FIG. 6.

leave the focal cavity is inhibited by the repulsive action of the closely adjacent cathodal point, so that only few electrons escape from the cavity. Pinhole camera pictures demonstrate a very considerable amount of secondary radiation inside the cavity, while only a comparatively small intensity of "stem" radiation is to be observed.

18(*d*). If a radiation of two or more degrees of hardness is to be generated then it is easy to introduce two or more pointed cathodes into the same tube (Fig. 6), and grind the different points to different angles (Fig. 4). In this case either a single cavity on the anode is to be used, so that electrons of different points may be caused to impinge nearly on the same area or there may be provided a particular cavity for each particular point (Fig. 7). Experience shows that for most radiographic and fluoroscopic purposes no more is needed than a single point. However, since two degrees of penetration are required for the general diagnostic work, a series of tubes have been constructed to meet this demand. Tubes for deep treatment are also provided with two points, the softer one

FIG. 7.

being used in the first stages of exhaustion, while provision is made to later remove this auxiliary point from the space of the discharge as well as from the angle of the radiation which is to be applied.

19(*e*). One of the characteristic features of the tube is a particular member attached to the anode for the purpose of glowing out the cathode or the cathodes by electronic bombardment during exhaustion. It is a small piece of tantalum or molybdenum foil (*p*, Fig. 3) indented on one of its edges so as to provide a number of sharp points and mounted on a 2 mm. wire, like a flag on its shaft. While evacuating the tube, the flag is facing with its indented edge the pointed end of the cathode. By loading the anode negatively, an electronic discharge is started, so that not only the pointed end, but also the shaft of the cath-

ode, including the small disc (*d*) receives electronic impact and may be heated up to any temperature required. After sealing, the tube is inverted and by gentle tapping the flag is caused to slide back along the anode stem, where it is definitely fixed by a small metal clip (*g*) in which position it is represented on Figure 8.

20(*f*). Special attention has to be paid to the evacuation, because a higher degree of

FIG. 8.

exhaustion is required than in the case of the hot cathode tubes (4). For the purpose of improving the vacuum, especially to absorb gases developed by the sealing of the tubes, a chamber (*c*, Fig. 9) can be provided in which any metals fit for the purpose may be sputtered. The chamber consists of a small bulb of 3 cm. in diameter, in which a cathodically active point is mounted, facing a plane anode of the material to be sputtered. While connected to the pump the electrodes are treated in the regular way, in order to get out as much as possible of the occluded gases. After sealing the tube, a sufficient layer of sputtered stuff is produced and since afterwards the electrodes are no longer to be used any more, they are covered with protective caps.

21(*g*). I have not yet been able to determine satisfactorily the electric properties—first of all the characteristic—of the tube; a plant delivering a constant direct current of high voltage being needed for the purpose and only little time being available, I was obliged to postpone a more detailed research of this subject, as well as of many other rather important features. But there is no doubt that a rapid increase of the milliamperage with a rising voltage takes place, probably an exponential function (10). Evidently, if worked by alternating or pulsating direct current, the

FIG. 9.

discharge is limited to the peaks and the adjacent parts of the waves of the voltage to a larger extent than is the case with any other of the present tubes: surely, to a very much larger extent than with the regular hot cathode tubes, in which the milliamperage is practically independent of the voltage. Consequently, the same spark gap being supposed, not only is there obtained a considerably higher average hardness of the radiation, not only a much larger intensity of radiation per milliampere, but also a much more homogeneous quality of radiation. The degree of homogeneity can be regulated by choosing proper forms—different numbers of points, different lengths of the edges—of the cathode.

22(*b*). While the constructive features as described are essential and will probably remain typical with the tubes constructed according to the principle developed above, the same cannot be said of the external form of the tubes. Though the tube shown in Fig. 9 can be fitted in most of the usual holders, probably different types will be developed for particular purposes; a rectangular type for dental use, a straight type for radiography, and finally a special form for deep treatment purposes. Various cooling devices may also be introduced.

DISCUSSION

DR. LEVIN. I would just like to ask Dr. Lilienfeld whether his tube was tested for the high voltage machine that is being used at the present time with 2½ ma., and whether it is possible to get the same conditions, because I believe if this tube were workable it would be superior to the gas tube on the one hand and the Coolidge tube on the other, because it does not require the additional filament control, etc. The doubt in my mind is whether it will always be possible to get those same conditions, or will it all of a sudden stop giving the roentgen ray and simply spark across.

DR. SCHMITZ. I think about three or four years ago some research work was done by the Western Electric Co., and it was determined that a great deal of radiation was obtained from the arms of the Coolidge tube. The benefit I can see in the Lilienfeld tube is this: there is practically no radiation from the arms of the cathode and anode. Whether it is possible to use the Lilienfeld tube in therapy, I do not know. I have not seen this tube used for that purpose. I can see the advantage of using this tube for roentgenographic work, provided you are not using the Potter-Bucky diaphragm. If you use it, necessarily you do away with the radiation that is obtained from the arms of the tube. The question raised by Dr. Levin as to whether the Lilienfeld tube could be operated on high voltage is a very interesting one. If not, of course, it could not be used for therapy at all.

DR. LILIENFELD (closing discussion). As far as Dr. Levin's question goes, I can only state that of course there is a possibility of applying the tube for deep therapy. The application of the tube for deep therapy is given, for the reason that the whole radiation is concentrated, with the exception of only very little percentage, up to the real focus point. If a tube is constructed for 100,000 volts, the discharge will start at 70,000 volts and go up to 100,000. Now, as to the question in regard to how high voltages have been used, I have to say that while I invented the tube I discovered that it was better not to use high voltages but to show that even low voltages will do the trick and will cross over the gap between the point and anode. Hence to me the most stupefying observation was that voltages of very few thousand volts would do the trick. There was no difficulty whatever in making the gap larger and of working the tube at 100,000 volts or more than that. I have no doubt that any voltage required can be adapted to this tube. You have to look at this particular invention from the historical standpoint too, because you know perfectly well that the ionization, which was the basis of the first tube, was known for sixty or even more years before definite types of gas tubes were made. The effects of hot cathode were discovered by Edison forty years ago. A great amount of detail work has been done for about thirty years, and only afterwards came the hot cathode roentgen-ray tube. Now this electronic phenomenon is very young. Only two or three years ago I had the first conception that such a thing could be realized. Of course, in these three years I was not able to do the whole development work which will be definitely required. I hope that the definite treatment tube will be made in about six or eight months. At any rate, I do not see any reason why higher voltage could not be applied for these tubes.

STEREOFLUOROSCOPY*

BY J. D. MORGAN, M.D., B.A.

Roentgenologist, Ross Pavilion, Royal Victoria Hospital

MONTREAL, CANADA

TWO *x*-ray tubes, with targets placed several inches apart, are alternately excited. These short alternating flashes produce different images on the fluorescent screen corresponding to the difference in position of the tube-targets. Owing to the persistence of each image in the eye, the result is a confused mixture of the two images. If, now, a shutter be placed in front of the eyes, and so arranged as to permit the eyes alternately to see the screen in synchronism with the flashes of the two tubes, each eye will see but one image, but the observer will be conscious of a continuous stereoscopic effect. Very early in the evolution of roentgenography, stereoscopic roentgenograms were an accomplished fact. The interest they aroused was naturally followed by attempts at stereofluoroscopy. One of the earliest to attain success was Mackenzie Davidson, of London. At a meeting of the Roentgen Society, held on December 6, 1900, he gave an exhibition of his stereoscopic fluoroscope. The shutter consisted of a rotating disc with appropriately placed slots which eclipsed each eye alternately with the sparking of the tubes. The President, Dr. MacIntyre, in thanking Mr. Davidson in the name of the Society, remarked that he thought the apparatus perfect in principle, but, in its present form it seemed to him it would be difficult to get patients to adapt themselves to the necessary position. Mr. Davidson (it will be remembered that he later became Sir James Mackenzie Davidson) himself remarked that he feared its cost, which must of necessity be high, would preclude its use from those working in hospitals with small means.

In 1911, our friend Dr. Pirie devised a much more portable apparatus which he described in the November issue of the *Archives of the Roentgen Ray* (London). His shutter, he wrote, "resembles a lady's hand-mirror in shape and size." It consisted of a perforated disc which was rotated by means of a flexible shaft connected at the other end to the main shaft of a Snook machine which latter also served to activate the tubes. Dr. Pirie made note of the important fact that "when using two tubes it is necessary to have them of the same hardness" and recommended the use of a single *x*-ray tube with two anticathodes, such, for example, as that made by Bauer. Attempts were also made in France and Germany to accomplish stereofluoroscopy. A description of a French apparatus is given by Lievre. I have not, unfortunately, been able to obtain details of this apparatus.

In America, the best known stereofluoroscope is that devised by the late Dr. Caldwell. It was fully described in the December (1918) issue of the AMERICAN JOURNAL OF ROENTGENOLOGY. The shutter has three wings which serve to cut off the vision from the eye-holes. It is driven "by making it the rotor of a motor, the field windings surrounding it and placed just beneath the face plate." To obviate the difficulty of keeping two gas tubes to an equal degree of vacuum, Caldwell made use of two self-rectifying Coolidge tubes. Dr. Williams, even prior to this, had devised an apparatus with a revolving shutter. This apparatus may still be seen at the Massachusetts General Hospital in Boston. Another recent English apparatus is one in which, again, the shutter consists of a disc which is rotated by means of a long flexible shaft, similar to the one devised by Dr. Pirie. The objection to this type of apparatus is the difficulty of retaining synchronism, owing to the "give" in the flexible shafting. In order to overcome this objection an attempt was made several years ago, by the late Dr. Robert Wilson and Mr. L. R. McDonald, my associate in this work, to construct a synchronous motor in direct contact with the revolving

* Read at the Twenty-Second Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Washington, D. C., Sept. 27–30, 1921.

shutter (Fig. 1). This created another difficulty, however, namely, that of weight. Mr. McDonald's apparatus, the lightest I have seen of this type, weighs about fifteen pounds. A further objection to all these types of stereofluoroscopes is that they occupy at least one hand of the operator, thus limiting his movements. Or, if the shutter be suspended, thus freeing the hands, the field of operation is limited.

FIG. 1. Motor and shutter mounted on handle and connected by very short flexible shaft.

Yet another American stereofluoroscope is described by Dr. Heck, in the issue of the AMERICAN JOURNAL OF ROENTGENOLOGY for September, 1920. Dr. Heck uses two rotating shutters. He also uses two Coolidge tubes, each with its own transformer and control. The shutters are connected through suitable gearing to a solid shaft on the other end of which is a twelve-inch arm whose extremities travel close to two semicircular rods, each of which is connected to one of the Coolidge tubes. An electric motor drives this system at a speed of 900 revolutions a minute. By this means the eye gets fifteen light impressions per second. The objections to this apparatus would seem to be its size and lack of flexibility. In the July, 1921, number of the *Journal of the Roentgen Society*, London, is a description of an apparatus designed by Prof. Tyndall and Mr. Hill. Their objects have been simplicity and economy (Fig. 2). They, therefore, use but one Coolidge, or gas tube. This is mounted on a suitable insulated carriage and attached to one end of a vertical metal strip the other end of which is passed over a pin (*p*), which itself is fixed eccentrically to the face of a circular disc (*c*). "When the disc rotates the pin (*p*) forces (*m*) and the carriage to which it is attached to execute a linear oscillation through a dis-

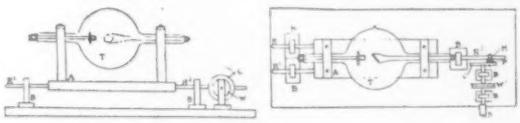

FIG. 2. Views from the side, and from above, of the tube in carrier.

tance equal to the diameter of the circle described by (*p*). This can be varied by placing the pin in any one of a series of holes at different distances from the center of the disc (*c*)." An excursion of about one inch was found to be the most efficient, and a speed of about ten revolutions a second to give satisfactory persistent pictures. Under these conditions no undue strain is placed on the tube. A rotating shutter is used (Fig. 3). My criti-

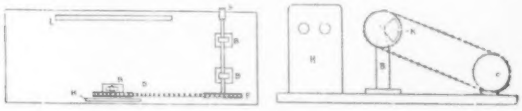

FIG. 3. Views from above, and from the side, of the rotating shutter.

cism of this apparatus, without having seen it, would be the cumbersome chain drive of the shutter and consequent lack of flexibility.

Finally, we come to the apparatus to which Mr. McDonald and myself have devoted considerable time during the past eighteen months. It differs essentially from the previous types I have described in that the shutter has a vibrating, instead of a rotating, movement. Several previous attempts have, I believe, been made to construct such a shutter, but without complete success owing to the inability to synchronize the vibrations with the lighting of the tubes. The fault has been in the use of forced vibrations. In our shutter the vibrating member is placed

between two specially made electromagnets and only enough force applied to overcome inertia and air friction. It, therefore, oscillates in a natural manner. Two Coolidge tubes are used, both activated by the same transformer.

Further advantages of this apparatus are: First, its small size and lightness—it weighs only twelve ounces (Fig. 4); second, it can be strapped to the forehead

FIG. 4.

like a head-mirror (Fig. 5), thus leaving both hands of the operator free; third, when not in use it can be pushed up from in front of the eyes without removing it from the forehead; fourth, it can be fitted with an "operating-fluoroscope," thus rendering it possible to operate in a fully lighted room; fifth, it can be used equally well in any position without losing synchronism; sixth, a number of observers, each with a shutter, can "plug in" and view simultaneously the shadows stereoscopically, as they appear on the common fluorescent screen. The accompanying pictures are of our homemade apparatus.

DISCUSSION

DR. HICKEY. I had the pleasure of seeing the stereofluoroscope of Dr. Morgan in operation very shortly after it was brought out. I was much impressed with the simplicity of it. The only disappointment is that we cannot get one. Personally, I feel that the stereofluoroscope is something which the manufacturers have neglected. I have been very much interested in the manipulation of fractures under the fluoroscope in the hospital with which I am connected; practically every fracture is reduced by fluoroscopic control. The inconvenience of having to turn the patient over to judge of the position in the lateral direction so as to be sure we are not getting a misconception of the position, is something I think which could be overcome by the stereofluoroscope.

DR. PIRIE. I have had the pleasure of seeing this stereoscope working and thoroughly appreciate what a beautiful instrument it is. It is better than former stereoscopes in

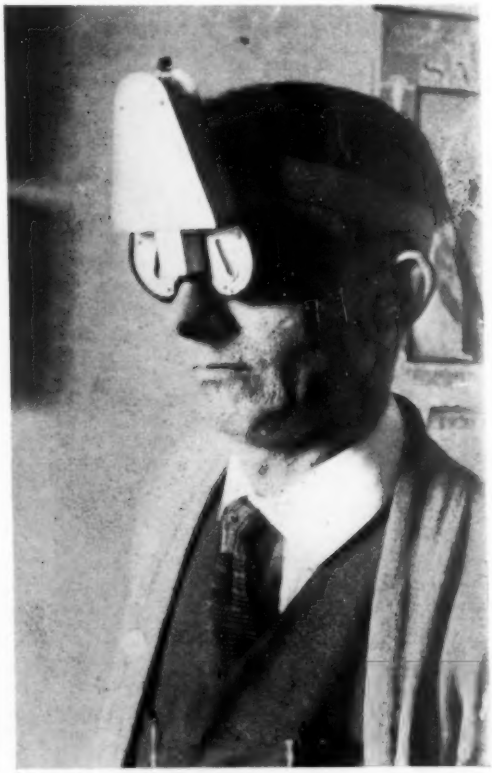

FIG. 5. The shutter can be pushed away from in front of the eyes without removing the strap.

that it is much lighter. It works perfectly and leaves your hands entirely free. I do not see that we want anything better.

The only disadvantage of the stereofluoroscope is that you have to have a special apparatus for it or you have to set it up every time you want to use it and take it away again when you want to use the single tube.

It gives a perfect stereoscopic effect and it is the best thing in stereofluoroscopy I have seen so far.

DR. MORGAN (closing discussion). I think the point brought up by Dr. Pirie regarding the difficulty of changing over from using double tubes to the use of a single tube is a good one,

but any objections of this sort I feel sure will readily be overcome when once the manufacturer will have taken up the matter seriously. This is a question of mechanics, which, I think, will prove, in the end, not to be as difficult of solution as may now appear. I can see no reason why some form of apparatus cannot be constructed having two tubes for stereoscopic work, and with some arrangement by which one tube could be cut out when one wishes to do ordinary roentgenographic or roentgenoscopic work. This stereo work is only in the experimental stage. I do not know that the last word has been said regarding the design of a shutter but our hope is that with our present model we can very considerably reduce not only the great bulk and inconvenience of previous types, but also the cost. We, also, have tried to design something which could be sold independently of the rest of the apparatus so that, with one of these shutters, and a transformer already in his possession (remodelled at small cost), a doctor can readily undertake stereoscopic work.

NEW TECHNIQUE FOR THE VERTICAL EXAMINATION OF THE SPHENOIDS AND ETHMOIDS, WITH DEMONSTRATION OF SPECIAL SPHENOID FILM HOLDER*

BY GEORGE E. PFAHLER, M.D.

PHILADELPHIA, PENNSYLVANIA

MUCH difficulty and dissatisfaction has been encountered in the demonstration of the sphenoids and ethmoids by vertical view; that is, in the demonstration of the horizontal plane of these sinuses, so as to project them side by side in horizontal section. Heretofore it has been necessary to either project the outlines of these sinuses through the vertex of the skull or downward within the submaxillary space and below the tissue of the neck. In either instance the position of the sinuses is so far away from the photographic film that there is much distortion and great want of definite detail. With the object of bringing the films nearer to the sphenoid and ethmoid cells to be photographed I have devised a method of placing a special film in the mouth, pushing it backward firmly against the pharynx. This gives a definite level for the projection of the outlines of these sinuses and eliminates most of the irregular extraneous shadows of overlying bones. One has then above this film only the base of the skull, and in this small area one practically obtains only the outlines of the sphenoid sinuses and the ethmoid cells surrounded by a border of teeth in the upper jaw. A preliminary report on this method of study was made by me before the American Roentgen Ray Society in discussion.[1]

Many difficulties have been involved in this procedure. In the first place it is necessary to cut the films to the exact size and shape that will fit the average mouth and pharynx. I obtained this first by finding a size that would best fit my

FIG. 1.

own mouth and which I could hold in position comfortably when pushed entirely against the posterior wall of the pharynx. This size is 2 in. by 3 in. It is square at one end and curved at the end which is pushed against the pharynx. I then found great annoyance from the secondary radiation of the tissues about the mouth which caused a great deal of fog. This was partially overcome, even as early as 1916, by the use of metal placed

* Read at the Twenty-Second Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Washington, D. C., Sept. 27-30, 1921, and previously at the meeting of the Section of Laryngology of the College of Physicians of Philadelphia, Mar. 16, 1921.

[1] The annual meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Chicago, Ill., Sept. 27-30, 1916. AM. J. ROENTGENOL., Aug., 1917, iv, 411.

underneath the film, yet there was still much disturbance from the secondary radiations of the tissues of the head because it was necessary to use, as in all vertical examinations, a comparatively hard ray. More recently I have overcome this by using double screens which I have cut to exactly fit this special film. The double screens are attached by means

FIG. 2.

of a hinge, the lower one being attached to a layer of brass (Fig. 1-A). Special black paper envelopes are made to fit these holders and they are rendered waterproof by covering them with rubber. This double-screen technique permits the use of much softer radiation which gives rise to less scattered rays (Fig. 1-B).

This examination is made with the patient in the sitting posture (Fig. 2). The chin is rested upon a headrest. The film is then pushed back into the mouth against the pharynx and held in place by the teeth biting firmly. The distance from the target to the top of the head is 18 in., using a 3 in. cylinder and a vacuum corresponding to a 4 in. parallel spark gap, and an exposure of approximately eight seconds, with 30 ma. of current.

By this technique we can now demonstrate clearly the outline and size of these sphenoid sinuses projected side by side and this gives the operating surgeon a definite idea as to the position of the septum, for in many instances the septum is distinctly to the right or the left of the

FIG. 3.

median line. By this process one can also demonstrate a horizontal projection, or in horizontal section, the ethmoid cells. At times large ethmoid cells in the region of the sphenoids are involved by exudate which, by all other means, leads one to suspect disease of the sphenoids. With the definite demonstration of the location of exudate by this means, or even the demonstration that these various cells are normal, I am sure that a great advance can be made in the study and treatment of the diseases of the posterior accessory sinuses.

By this method I have been able to demonstrate surprisingly great variations in the outline of the sphenoid sinuses as well as the great variation in size and outline of some of the posterior ethmoid cells. It is rare that the two sphenoid

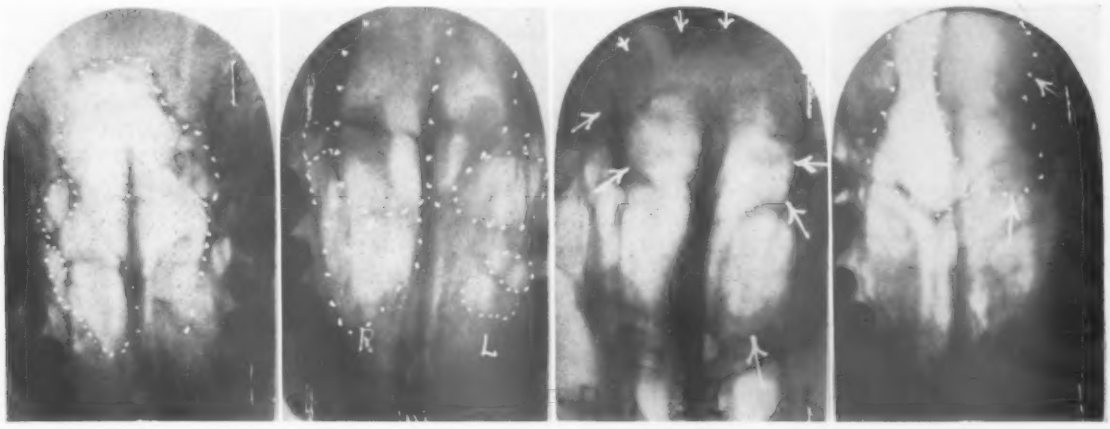

FIG. 4.

FIG. 5.

sinuses are of equal size and it is very common to find the septum on one or other side of the median line. This technique combined with the postero-anterior and the lateral views permits one to make a very exact and definite demonstration of the sphenoid sinuses in every plane and in every direction, and therefore furnishes the most exact information for the clinician (Figs. 4 and 5).

DISCUSSION

DR. HICKEY. I would like Dr. Pfahler to give us the approximate angle when you look at the patient from the side

DR. HEBERDING. I would like to ask Dr. Pfahler if he finds it necessary to use a local anesthetic to prevent agony.

DR. PFAHLER (closing). First, Dr. Hickey's question. I do not believe we can establish any definite rule about the side angles—about the direction you send rays down—because you must judge where the sphenoid is according to the shape of the head. In a general way, it is in a plane 2½ cm. anterior to the external auditory meatus. Therefore, you find that point on a level with the auditory meatus, then project the central ray toward the mouth and you get the sphenoid on the film. It is just keeping in mind where your film is and realizing the position of the sphenoid, and then you project downward and forward, as shown in final illustration.

As to the anesthetic, no. Neither have I had any objection on the part of the patients, which is surprising. The film is dipped into ice water just before inserting, and I have never had any one gag or push it out. It takes only a few seconds. I have been using 4 to 4½ in. spark gap, 30 ma., about eight seconds, exposure at 20 in. distance, or about 16 in. above the top of the head. I do not believe the majority of you will need to use that much exposure. I am probably using a little weaker developer. I use a 3 in. cylinder for my diaphragm.

As to the manufacturer, there is no special manufacturer. This was made by my own personal mechanic, Thomas Gallen, 1939 Wood St., Philadelphia, Pa.

DENTAL ROENTGENOGRAPHY IN THE LIGHT OF CLINICAL AND PATHOLOGICAL FINDINGS*

BY ALLAN SCOTT WOLFE, D.D.S.

WASHINGTON, D. C.

IN addressing you today, I am not unmindful of the honor paid my profession. I believe those of us who are specializing in oral elimination have a place in the scheme of diagnostic and preventive medicine. We should be encouraged by you gentlemen; since only by our operations and study of pathology can you be assured of a check on your roentgenographic interpretations of films taken in the mouth.

The general practitioners of medicine and dentistry, who take oral films and after a casual glance advise a patient as to the retention or elimination of teeth, are still in the majority. A great many of these men know nothing concerning the anatomy or histology of the mouth but still pretend to diagnose conditions of pathology of these parts and of the entire body.

Many expert roentgenologists are in a similar predicament. They know their anatomy and histology but not having the benefit of mouth operations, ante-mortem or post-mortem, they have gone from year to year building up certain forms of nomenclature and certain set plans of diagnosis for conditions which may or may not be found in the mouth.

Before going any further, I would like you gentlemen to remember that I am not insisting that the principles laid down in this paper are positive. I am merely giving you my findings in the elimination of oral foci and asking you to apply them at some time in your practice so that eventually we may arrive at some definite mutual conclusions.

There is a saying which tells us to strive for what is right, and not for what is expedient. This, to my way of thinking, should be the motto before us in the interpretation of the oral film. If it were a matter of expediency I would try to produce something startling for you gentlemen; as it is, I must ask your pardon for starting in on the most elementary roentgenography. It is only by so doing that I can arrive at definite conclusions.

We are told by the masters of the various arts that only by comparision can we tell the good from the bad and the true from the false. If that be so in the world of arts and letters, is it not doubly so in our work? Should we not study the anatomy of the parts just as faithfully as the pathology, comparing the two in all cases so as to get the best results? This is very seldom done, especially by the practitioner of dentistry. The reading is usually given on pathology alone, completely overlooking the anatomical variations in this particular mouth, which may have more bearing upon this individual case than the pathology shown.

Let us now get down to the basis of this paper which is a simple classification of the morbid conditions in the oral cavity.

Apical Infection	Circumscribed Areas
	Diffused Areas
Pyorrheal Infection	Local
	Systemic
Nerve Irritants	Impacted Teeth
	Hypercementosed Teeth
	Retained Cysts or Residual Infection
	Pulp Nodules
	Hidden Cavities

Added to these we have the lower border of the maxillary sinus and injury to the maxillary bones such as fractures. This constitutes a classification that can be applied to all morbid conditions and anatomical variations of the mouth as shown by the oral film.

Let us now take up some of the terminology heretofore used in describing mouth conditions: *Radio-lucent areas; radio-opaque areas; rarified areas; abscessed areas; diffused areas; infected areas; granuloma; cyst.*

* Read at the Twenty-Second Annual Meeting of THE AMERICAN ROENTGEN RAY SOCIETY, Washington, D. C., September 27–30, 1921.

These are but a few of the terms used to explain the different shadows we find at the apex of pulpless teeth, and compared with the actual conditions in the bone they mean nothing to the operator who is supposed to eliminate them.

Let us assume now that we have gotten beyond the common errors usually made in reading the dental film, such as foramen, nostrils or large bone cells, interposed between tooth and film, and that we are reading a film of a pulpless tooth. Is it not good practice to compare this film with one taken from the opposite side of the mouth? I am assuming, of course, that the whole mouth has been *x*-rayed as one, and that two or three films are not enough on which to render an opinion. We now have an opportunity to study the bone cell and the peridental membrane of vital teeth in the same mouth with pulpless teeth. We may find this membrane thick and heavy over one tooth and not over another. Is that a sign of infection? Not necessarily. It is probably trauma caused by a high filling or a poorly fitted crown. Or again we may have a fistula discharging from a foramen located away from the apex of the tooth, leaving this apex seemingly in good condition although slightly clouded. In many cases this is called by the roentgenographer a slight infection. When we make a diagnosis of this kind it is of very little value. We must remember that infection does not show on the film. What we see is the result of infection which is destruction of tissue, both hard and soft.

There are many times when we find teeth which show no apical disturbances but which have all the clinical symptoms of infection, and when extracted by the most approved methods show decided pathological conditions in the laboratory tests. On the other hand, certain authorities report large cysts taken from the mouths of healthy patients whose teeth have never been tampered with by the dentist, and have found these cysts to be sterile.

With these few comparisons I am going to make the following statement upon which my practice is based. In all cases of systemic infection or nerve irritation accompanied by lowered resistance, it is my practice to eliminate entirely all pulpless teeth which show a break in the peridental membrane, and also nerve irritants such as shown in the classification under that head, never venturing an opinion on roentgenographic evidence alone without clinical observation.

In taking up the subject of oral elimination, I feel that no apology is necessary for my position in this matter; and since I have spent most of my life in dentistry, I have come to the conclusion that a greater percentage of dental operations are failures rather than successes and that the average operation, especially root canal work, is a liability rather than an asset to the patient. Operations done in the mouth of healthy patients may show up well for a number of years, but there comes a period of slow disintegration, and it is only a matter of systemic resistance, until the malignant conditions triumph over the benign. The operation in which the dentist took such pride yesterday, is today a filthy mass of tooth, metal and decayed food. We must now reverse the process by strengthening the benign and eliminating the malignant. I do not hold with some of the gentlemen in the profession that infected teeth are the primary cause of all human ills, but I do believe they are secondary and contributory, and that when we have systemic conditions we do help the patient by the elimination of oral foci, thereby increasing the resistance of the body.

We will now take up the condition of apical infection. As I have said before, there are only two conditions of infection that the oral surgeon finds at the apices of pulpless teeth—the encysted or encapsulated, and the diffused areas. Now let us study the cause of these two conditions.

We are hearing every day about the pendulum swinging back—that the extraction of teeth must cease—but this will not be until the present crop of diseased teeth is eradicated and the dental profession taught to conserve the pulps of teeth or be more scientific in their treatment. Of all faulty technic in any profession the treatment of pulpless teeth is probably the worst, and the more thorough the treatment the more disastrous the result. I can show you oral films of tooth roots

half filled, with the apex in perfect condition, where upon extraction the root ends present the normal condition found in a vital tooth. But practically all the tooth roots that have been filled are infected and eroded through and through. There is no technique in dentistry that will sterilize the putrescent condition of a canal and adjacent bone and keep it so for years without injury to live tissue. The dentist may pack such teeth full of silver nitrate and formaldehyde, trusting to luck, but the result is inevitable. The teeth are tolerated by the system until resistance is overcome, possibly by a combination of causes, and it is at this time that the patient presents himself.

As to root canal treatment, do not misunderstand me. I believe that simple straight-rooted teeth can be treated and saved for a great many years without harm to the patient, but I do not believe in the retreatment of teeth; and in view of these facts, when the patient presents, if we err, let it be upon the right side and over-eliminate rather than under-eliminate.

The encysted area is nothing more than a slow-growing mass at the apices of pulpless teeth—a bone cavity encapsulated by the resistive forces of nature, the contents of which may be granuloma or cyst, sterile or infected; it makes very little difference. It should be removed, after extraction, with a dull curette, otherwise, without the elimination of this, the extraction of teeth is useless.

In cases of diffused areas the roentgenogram shows no limiting wall, but a feathery shadow, extending from the root ends into healthy bone. In the mouth, this bone presents a necrotic condition with little vascularity until normal bone is reached. A sharp curette should be used to remove this bone, not only to get rid of the infection but also to stimulate cell activity. This is nothing more than a chronic condition of devitalized bone, permeating the trabecula to an unknown extent. One who has made numbers of root resections and Novitisky's operations will recognize the difference between this condition and the encysted areas where we have a healthy condition of the bone.

As to the cause of the diffused area I have my own records and the past history of the patient to rely upon; also an intimate knowledge of the methods used by various dentists who have practiced upon such patients. From these records I am convinced that the diffused area is partially due to arsenical poisoning. Why the dental profession has tolerated the use of arsenic as a devitalizing agent for so long a period, I cannot understand. It is a corrosive poison, the action of which does not stop at the apex of the tooth but keeps on going through the trabecula until we have liquefaction of bone. This is really one of the greatest causes of oral infection and the simple extraction of teeth is not enough to eradicate it entirely. I do not think that we have this diffused area in all cases where arsenic has been used, but I believe it accompanies lowered resistance and anemia; in other words, if the destruction of bone is too great, diffusion will result. Another cause of this condition is teeth devitalized through trauma in young patients, or those with very low resistance. We very often find large diffused areas in apparently good teeth of which the patient can give us no history.

PYORRHEAL INFECTION

We are told that pyorrhea is the liquefaction of bone due to local and systemic conditions. Here again it remains for the oral surgeon, in doing research work, to apply or have applied all known procedure in the treatment of this condition, checking up from time to time with the roentgenogram, smears from the mouth, blood tests and urinalysis. When this has been honestly done, it will be found that if the disturbance at the crest of the alveolar process comes from lack of contact, contour and poor occlusion, that is, from purely local conditions, regeneration will take place when proper remedies are applied. On the other hnad, when conditions are systemic, and the cancellated bone surrounding the tooth is pus-soaked, this condition occurring all over the mouth irrespective of contour or contact, then we have the true systemic condition of pyorrhea, which treatment, up to the present time, has never been able to conquer. Any type of treatment is merely

temporizing; and it is only a short space of time until wholesale extraction is resorted to, in order to relieve the patient of a filthy condition which, to my mind, is only a sympton of some unknown systemic condition.

What is the duty of the roentgenologist in these two cases? For type one, where the loss of process is due to the impactions of food, poor contact and occlusion, it is the duty of the roentgenographer to advise complete restoration of the dental arch supplying all teeth that are lost and replacing all fillings that are faulty. Also in this class we may have just the opposite condition—the third molar, erupting without enough room, will distort and buckle the arch, thereby causing a condition of pyorrhea. I have seen many cases where the early extraction of the third molar would have meant the saving of the entire denture. In the treatment of type two, I have had nothing but failure, and have seen the same results from the work of the most skillful of operators. The patient's judgment cannot be considered in this matter, as he is over-enthusiastic at the apparent results obtained in the first few treatments, and, wishing to retain his teeth, the reports he gives are untrustworthy. If the physical diagnosis shows any alarming condition the infected teeth had better be eliminated. Remember, we are now speaking of the patient who comes to you for some systemic disturbance accompanied by lowered resistance, which I understand constitutes the bulk of your work.

NERVE IRRITATION

The condition of nerve irritants from impactions of teeth, hypercementosed roots, retained cysts, pulp nodules and and hidden cavities should now be considered. For years, patients have been told that impacted teeth caused no trouble. Since the appearance of the *x*-ray, the dentist who is afraid to eliminate this form of nerve irritant has had a hard time convincing both diagnostician and patient that these impactions were harmless. In the light of recent investigation we know that excess irritation, such as that of the third molar, with the crown impinging against the gingiva of the second molar causing peripheral irritation, and the roots depressing the inferior dental nerve and vessels, causing a dilatation of the erectile tissues of the turbinated bones through the sphenomaxillary ganglion, are conditions referred back to the face, neck and shoulders as different forms of neuralgia and neuritis. The same condition holds in hypercementosis and retained cysts, especially near the mental foramen or any nerve trunk. It will be noted that the bone cells around the cyst or impaction have lost their form and become flattened. This is caused by the gentle pressure exerted upon the cell while the cyst is forming or the tooth growing. In many instances, especially in the lower jaw, this pressure continues through gravity down to the nerve trunk causing a definite neuralgic condition. These conditions cause as much, if not more trouble, than infection on root ends, but of a different type. So I believe that the tooth, cyst, or hypercementosed condition, in attempting to find an outlet, natural or otherwise, presses against the nerve trunk or periphery causing referred pains that are purely mechanical and have nothing to do with infection or infiltration of pus. It is in these cases that we have the so-called "quick cures" following elimination of the irritant. In cases of infection it is months before the benign forces of the body overcome the malignant and we have resolution or, if infection be too great and resistance not high enough, we have dissolution. It is for these reasons that I say the old-fashioned extraction of teeth is one thing and the modern surgical elimination of focal infection something quite different. It is because we have had the old-fashioned method in the past that we are now having the retained cyst to contend with. This is a condition where the granuloma or cyst is left intact in the bone cavity and continues to increase in size; and for that reason, spaces where teeth have been extracted and edentulous mouths, should be subjected to as close a scrutiny as if the teeth were present. In many instances the diffused area is present after extraction of teeth, but if resistance be high this condition is absorbed much more readily than the encapsulated area. Treatment of this

condition is by thorough curettage and packing of cavities, care being taken that healing is not by first intention.

Neuralgic conditions caused by hypercementosed roots, sometimes misnamed exostosis, and impacted teeth can be very often diagnosed by deep injections of novocaine. In many cases after *x*-ray diagnosis has disclosed the presence of an impaction in cases of trifacial neuralgia or "tic," nerve-blocking has given instant relief and the subsequent elimination of this particular form of nerve-irritant has effected a cure.

The question of pulp nodules is one that I approach with a great deal of hesitancy. Many cases of neuralgia which I have felt sure were traceable to this form of irritant, have turned out to be something entirely different. The roentgenogram does not give a positive diagnosis of pulp nodules, for in the examination of extracted teeth I have found just as many pulp nodules which did not show up in the film. While I do not care to commit myself upon this very important phase of diagnosis, I believe that the only condition where pulp nodules are the cause of pronounced pain is where the pulp is becoming devitalized through decay, or under a large filling, and that the deposition of lime salts, which we find in perfect teeth and call pulp nodules, is a normal condition which does not justify extraction or elimination of the pulp.

Another reason for *x*-raying the full denture in cases of neuralgic conditions is the hidden cavity, usually beyond the gingiva of the molar teeth, which, undiscovered by the dentist and constantly irritated, it sends its complaint to the nerve centers. This simple condition causes as much trouble as anything we find in the mouth and can usually be detected with the oral film, although at times it may be read as a cement filling. It is impossible for the patient to localize this trouble in any certain tooth, and to my way of thinking it demands as much thought and work as pathological conditions.

MAXILLARY SINUS

In taking up the maxillary sinus I feel that I am straying somewhat from the field of the oral film, as in no case should a reading be made of the antrum from anything but full head plates. Here again it is a question of comparison in interpretation.

The average dentist or oral surgeon has neither the facilities for taking such films nor the ability to read them, but he must be taken into consideration in the elimination of antrum conditions. Many of these cases are mainly catarrhal and become infected from nasal conditions remaining wholly dependent upon the rhinologist for treatment. In my opinion there are fully as many cases infected chiefly from just a dental cause. Therefore, in diagnosing these conditions, smaller films should be taken of the lower border of the antrum, and if there are any pulpless teeth entering into it or near the floor they should be eliminated in conjunction with the treatment of the rhinologist, for it is a rule in oral surgery that no devitalized tooth projecting into the antrum should be treated.

I realize that the curvature of the palate makes it very difficult to secure an oral film of the antrum which is in any way positive, but it is my opinion that operations on it should be at the lowest point for drainage, especially when pulpless teeth are involved. After a diagnosis of the infected sinus has been settled upon, the use of the No. 2 oral film, of the lower border of the antrum, without regard to tooth roots, gives one the best landmarks to follow in operating.

This subject of the maxillary sinus is one which has not been given as much consideration by the profession as it deserves. We might term "No Man's Land," from the floor of the antrum to the apices of the teeth. The extraction of teeth has not entered the antrum from the mouth and the rhinologist has neglected it from the nose; hence it remains for the oral surgeon to clean this tract up with a definite surgical procedure in which he can be greatly helped by the roentgenologist.

Taking up the subject of fractures, I have only one thing to say from the standpoint of the oral surgeon, and that is "never advise leaving a tooth, fractured root or piece of bone in the line of fracture."

In reading this paper I am assuming that the roentgenologist is not only taking films or plates of the conditions I have set forth, but is giving his opinion before action is taken by diagnostician, or surgeon. Consequently, he should be very careful to limit the number of teeth extracted at one sitting. A patient of low resistance should not have more than three or four teeth extracted at one time, thereby showing the system the benefit of antibodies instead of being overwhelmed by the mouth infections. I know of at least 2 cases where death has followed wholesale extraction. Again, if there are several vital normal teeth in the mouth, it is well to save them, for clasped temporary plates give the patient the benefit of what we term denture experience, after which they become used to full plates more readily.

In closing I wish to say that any of the various subtitles that I have presented could have consumed a whole evening's discussion, and that it will be readily seen that this paper is nothing more than a review of work done in my practice and in clinics. Judged by your standards it will probably not be called scientific, but I am asking you to believe that it is an absolutely honest presentation of what I have found in the mouth.

DISCUSSION

Dr. Darling. It is a great pleasure to me to be able to discuss Dr. Wolfe's paper. He has given us the benefit of his twenty-five years' clinical experience. We want to do what is right medically by the patients and we have a problem to meet in what the dentist will do after he gets our findings and after he has the patient entirely under his care.

Dr. Wolfe is not what you would call a dentist. He is an oral surgeon. Now the oral surgeon is interested in eliminating the focus of infection in the same manner as the medical man is. He has a few of the things which medical men have not got and we must learn to speak the same language, and it seems to be quite a difficult task.

In roentgen-ray interpretation we are mostly interested to know whether to extract or not. The dentist lays more stress on the clinical examination, but all clinical examinations are doubtful and you use the *x*-ray as the deciding factor; it is effective in many cases. Whether it shows granulomata or abscess, walled off or diffuse, we are not so much interested in, as whether it is the focus of infection. The oral surgeon wants to know the character of the lesion he is expected to remove; he wants to know the extent. We should have this in mind. Research seems to have shown that a tooth once infected is always infected. Even teeth that are not infected in the beginning, after cleaning out the pulp shreds by sodium-potassium or other root canal explosions (which they really are), soon become infected; also after the arsenical treatment which Dr. Wolfe has mentioned. Novitisky claims that a dead tooth after six months is an infected tooth. In fact, the dead tooth is now the battle-ground between the doctor and the dentist. They are pulpless teeth, and there is a great attempt to show they are living teeth, which they are not. The nutritional exchange for cell metabolism probably does not take place beyond the cementum.

Dr. Wolfe eliminates all pulpless teeth which show a break in the peridental membrane. With reference to causes of systemic infection and lowered resistance, the dentist attempts to determine the lowered resistance in a few minutes in a dentist's chair, which Grieves points out takes all the brains of the medical profession to arrive at the diagnosis. The dentist settles it in his own chair very often, and determines whether the tooth should be extracted or not.

I would like to hear Dr. Wolfe say that the more thoroughly a tooth is treated the more disastrous the results. I would give a great deal to be present in a New York dental society meeting and hear him make that statement.

I am going to take slight issue with Dr. Wolfe to get him to explain further about retained cysts. I do not feel that the curettage in all cases is a necessary routine. I understand that to be the practice of oral surgeons, but I cannot agree with them. In the seven or eight years that I have been interested in this work, I have found only one such encysted area. I feel that curettage which breaks down the walled off area that has been established by the repair should be left intact and that there is sufficient drainage brought about by the removal of the tooth. Later, after healing, in those few cases where a cyst remains, another operation may be undertaken. I want to emphasize what he points out as important things for the roentgenologist to note in his report, namely, hidden cavities. I would add to that, shell crowns which are large and irritating, and irritating fillings which have been thrown in without being filed off.

In the following year you are going to see around the country a very strenuous comeback on the matter of bone restoration and repair. Bone restoration is, in my opinion, only sclerosis of the bone. The root canal treatment has managed to introduce a few staphylococci which have been able to produce chronic osteitis with sclerosis. Oral surgeons have not proven bone restoration either roentgenographically or by research. They are going to bring that back hard as a reason for treating infected areas around the teeth. I would like to hear Dr. Wolfe take that up.

I want to say in closing, that we must strive to speak the same language. We should not only strive but we should yearn, for the benefit of the patient, to speak the same language.

Dr. Scott. I would like the privilege of presenting two rather unusual cases. The first was a woman fifty-five years old, residing in Niagara Falls, with tonic contraction of the muscles of the lower jaw. She had a filthy mouth and the contraction was to such an extent that it was impossible to get a film behind the teeth. Examination was made through the back of the neck and it was found that all of the teeth were surrounded by abscess cavities. The only teeth remaining were the central and lateral incisors and the canines on both the upper and lower jaw. She was advised to have the teeth extracted and was sent to the hospital for that purpose and the teeth were extracted. Promptly following, she recovered full function of the muscles of the jaw and was able to use her jaw for its natural purposes. Apparently she was making a complete recovery. About three days after the extraction, she suddenly toppled over dead. The physician thought death was the result of embolism.

Strange as it may seem, within a period of three or four weeks following this case, another case presented itself—sent in by a physician from the country. This woman was apparently in perfect health; she was about thirty-five years of age, and had an absolute tonic contraction of the muscles of the jaw to such an extent that it was impossible to pry open the teeth in the slightest degree. She had apparently a full set of perfectly normal healthy looking teeth, so far as inspection would show. It was impossible to get anything in the shape of a film between her teeth. This woman gave a history of the condition coming on suddenly during the night without any previous evidence of illness. Examination was made in the lateral, oblique, and posterior-anterior positions, and two of her teeth were found infected. After removal she promptly recovered normal use of her jaw. About three weeks later the dentist to whom she was sent for the removal of her teeth telephoned me that the woman again came in and had complete tonic contraction of the muscles of the jaw, as extensive, or more so, than in the first instance. This time we gave her an anesthetic and made a complete examination of the teeth. All the teeth of the upper left jaw had apical abscesses. Before making the examination of the teeth we also made sinus plates, which were negative. The affected teeth were later removed and the patient made a prompt recovery. Strange to say, the day before I came here the dentist telephoned me that this woman came into his office with a recurrence of the condition.

The question which presents itself to me is, how much of this condition was due to irritation as a result of absorption and how much was due to neurosis? In other words, in the latter case in particular, was it a case of simple hysteria? In each recurrence the contraction was absolute and after removal of the infected teeth the patient was apparently normal for a period varying from four to six weeks.

Since reporting the above cases I have again examined the latter patient under anesthesia and found the remaining teeth to be entirely sound, the condition of tonic contraction of the muscles of the jaw, however, promptly recurred after recovery from the anesthesia.

Dr. Edmonson. The tooth is about the smallest roentgen-ray made, but it is one of the most important things in roentgenography. I should like to ask Dr. Wolfe if he is able, with any definite accuracy, to ascertain whether or not a tooth is devitalized from a roentgen-ray examination. I would like to ask him the limitations in extraction from the standpoint of health and disease. In my town the dentists blacklisted me on account of my attitude, but I call it conservatism. I cannot help believing that every tooth and every condition should be thoroughly investigated and that it is good medicine to investigate the systemic condition before sacrificing a tooth. Of course, if you have diffuse infection locally and general systemic infection, you are justified in extraction, but I do not believe in promiscuous pulling of teeth.

Another thing I would like to ask Dr. Wolfe is in regard to his judgment relative to the resistance of a patient. I have known some fatal cases where a good many teeth were pulled at one sitting. I know of a dentist who pulls them out by the bucket-ful every day. I had a patient whose teeth had to be extracted and I advised her to go into a hospital and have them pulled one or two at a time. Her blood-pressure was 240. The dentist got by with it and pulled one-half of her teeth one day

and the other half the next. It so happens she is still living.

DR. CRANE. I believe that physicians and dentists are making a horrible mistake so far as the ultimate good of the patient is concerned. We must not antagonize or criticize each other but must get together like fellow members of the healing art, and we will have to use some sort of language that each can understand.

My standpoint on films is that no man is capable of making any sort of diagnosis simply from the examination of the film. Examination of the film plus the clinical examination allows us to arrive at a comparatively satisfactory diagnosis. You who are in the roentgenogram branch of the profession have people referred to you for certain plates and films, and when they come to you from a physician for examination of the abdomen, thoracic viscera, or for the osseous structure, they come after a clinical examination. You base your reading of the roentgenogram largely upon the foreknowledge that you have of the clinical examination or the object for which the patient has been sent to you.

When you make examination of the teeth, in most cases they are sent to you by the internist or physician, and if you try or attempt to make a conclusive diagnosis from reading the shadows of the teeth, you are doing something which no intelligent dentist would attempt to do, because we find many conditions in the mouth which are infective and which may be as great contributing factors as the most apparent thing in the roentgen-ray film, and which give absolutely no roentgen impression. So that in order to arrive at an intelligent diagnosis, it is necessary not only to have a good set of dental films but to have a thorough clinical examination. Clinical examination without the film is not of great value. The film without the clinical examination is not of great value. Taken together they allow us to arrive at a satisfactory diagnosis of mouth conditions.

With regard to extraction and non-extraction of teeth in which the pulps have been removed, it would seem to me that the only rational way to approach that subject at all is to say that any teeth which are the seat of pathology and which cannot be cured, should be extracted. Of course, dentists differ as to what pathology can be cured or eliminated without extraction, but where we have no evidence of infection either clinically or roentgenologically, the man who mutilates a patient needlessly by extracting teeth, whether in his opinion the patient is subnormal or not, is taking a grave responsibility on his hands. Just today I have examined films of some very poorly treated teeth, in so far as the roentgen evidence of root canal filling is concerned, which have a history of having been treated ten or fifteen years ago. The line of pericementum is as intact and normal as any found on vital teeth. There is no clinical evidence whatever in the patient's mouth of any infective process or pathology about those teeth. If those teeth were of great dental importance to the patient and if I were called in consultation even if the patient had a serious systemic infection, I would not advise the extraction of those teeth, because I am firmly convinced that they could not be a contributing factor in the systemic infection.

DR. WOLFE (closing). In response to Dr. Darling's question as to curettage, I cannot agree with the doctor. When we speak of curettage we must again go back to the terminology used in the paper. The encysted area demands blunt dissection of soft tissue only. The diffuse area which, as I have said before, is nothing but devitalized bone, and needs definite curettage with sharp curettes. The one thing that pleases the oral surgeon is to extract teeth with granuloma, or whatever you choose to term it, clinging to the root. He knows that it should not be there and would not willingly push it back into the cavity. If drainage is complete at all times, this would probably absorb, but in many cases healing is by first intention, the abscessed area is retained and becomes what we term "residual infection."

As far as bone restoration is concerned, I do not believe that it is restored in the presence of infection. A good deal of it is a deposit of lime salts. If you will use a correal trephine, you will probably find streptococcic infection at the apices of most of the treated teeth, especially where an escharotic drug has been used.

With reference to sacrificing teeth, I would not extract simply because a tooth is pulpless but would depend upon the condition of the lamina dura. Teeth are very valuable, and I do not believe in their needless extraction, but I do believe that in cases of systemic infection or nerve irritation that elimination should be just as complete in the mouth as we can make it, handing over to the physician a patient who is free from all oral sepsis.

I am afraid I will have to disagree with Dr. Crane. If the patient were dying—and we have such cases in pernicious anemia—I would certainly extract any or all the teeth if I thought that in any way the patient would be benefited,—looking at it purely from the viewpoint of the physician.

I wish to say that I save just as many teeth as I can and am more proud of the teeth I save than those I extract.

THE AMERICAN JOURNAL OF ROENTGENOLOGY

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TWENTY-THIRD ANNUAL MEETING THE AMERICAN ROENTGEN RAY SOCIETY

LOS ANGELES, CAL., SEPTEMBER 12, 13, 14, 15, 16, 1922

Headquarters, Meetings and Exhibits: Hotel Ambassador, Los Angeles, Cal.

HEINRICH ERNST ALBERS-SCHÖNBERG

On June 4, 1921, Heinrich Ernst Albers-Schönberg, for some time an honorary member of the American Roentgen Ray Society, died, after years of almost constant suffering and after several months of well-nigh intolerable pain.

He was born in Hamburg on January 21, 1865, the son of August Heinrich Albers-Schönberg and of Amélie, née Des Arts. He received his medical degree at the University of Leipsic in March, 1891. After first acting as assistant in the gynecological department of the Hamburg-Eppendorf Hospital, and finally as assistant to the surgeon, Dr. Sick, he began the private practice of general medicine in Hamburg in 1895. In February, 1897, he established, together with Dr. Deycke, an institute for roentgenology and shortly after that devoted himself entirely to this specialty.

In the same year he founded the *Fortschritte auf dem Gebiete der Röntgenstrahlen*, of which he remained the editor almost up to the time of his death. The second issue of Volume XXVIII which came off the press on June 9, 1921, had still been under his editorship.

The number of his contributions to roentgenology, ranging from short papers to a complete textbook, is approximately one hundred and forty. But before the days of roentgenology he had published eight papers on other medical subjects. Most of his contributions are instructive and important. Some are epoch-making, as, for instance, the compression diaphragm and the development of the technique for the demonstration of urinary calculi, the recognition of the effect of roentgen rays on the generative organs, and, following in the footsteps of Foveau de Courmelles, the introduction into Germany of deep roentgen therapy. One of his early papers

HEINRICH ERNST ALBERS-SCHÖNBERG.

on the therapy of gynecological lesions appeared in the *Transactions of the American Roentgen Ray Society* for 1908.

His contributions are all the more important because, progressive and enthusiastic though he was, he possessed cool judgment and thorough scientific and clinical training. He was a keen observer, punctiliously truthful in his statements, and not afraid to admit an error or retract an assertion if later experience proved him mistaken.

Having experienced early the disastrous results of work with roentgen rays, he constantly preached the need of ample protection. His laboratory at the St. George Hospital in Hamburg was one of the first where protective devices were installed. The rules and regulations established there for the proper protection of patients, physicians, nurses, assistants and all other workers are a model in their completeness.

Ever since he devoted himself to roentgenology he labored assiduously for its elevation to the rank of a medical specialty. It is not belittling the work of others if one ascribes this happy result largely to his organizing genius, his profound medical knowledge and his true scientific spirit. Thanks to his efforts roentgenology has become an obligatory subject in undergraduate work in German medical schools. In recognition of his attainments he was appointed Faculty Professor of Roentgenology at the newly founded University of Hamburg, which was the first appointment of this kind in Germany.

It is difficult for the writer to speak of his personality without resorting to superlatives. His kindness, his sterling character, his readiness to help and encourage the beginner as well as the veteran, were unbounded. His cheerful disposition, even amidst intense suffering, could not be broken. Visitors who saw him in 1920 report that, although he must have suffered great torture from his cancerous growths, he never complained and never let anyone feel what he suffered. It was characteristic of the man that, even in the last letters which he could but dictate, he never complained of his own sufferings and was still ready to discuss roentgenological problems. His interest in these problems and his devotion to science remained alive till the end; this is strikingly shown by his ante-mortem request that his autopsy record be published in the *Fortschritte*.

Those who had the good fortune of knowing him, will never forget him. One visit only was enough to impress his personality upon the visitor. In many ways he reminds the writer of our own lamented Walter Dodd: serene in the face of the inevitable, progressive but not hasty, enthusiastic but not hot-headed, a worshipper of the truth, a lover of all men, a hater of none, not even of his enemies. As Forssell has so fittingly said: "His memory will live, honored and revered, as long as the history of roentgenology lives."

L. JACHES.

ANNUAL MEETING

If one may judge by the reports already on hand, the forthcoming meeting at Los Angeles will be one which no one can afford to miss. If you have not responded to Dr. Watkins' letter concerning reservations for space for lantern slides and other scientific exhibits, please do so at once. While the space available is large, the committee desires to know in advance just how much space to provide.

CANADIAN RADIOLOGICAL SOCIETY

INVITATION TO CONVENTION

The Third Annual Convention of the Canadian Radiological Society will be held in Winnipeg in conjunction with the Dominion Medical Convention, June 20, 21, 22 and 23. The officers and members of the Canadian Society extend a most cordial invitation to American Radiologists to attend this meeting. At the last convention the Society was honored by the presence of a very representative body of members of your Society, and we shall look forward to welcoming them and others to the Winnipeg meeting.

It is requested that those who contemplate attending, communicate at their earliest pleasure with Dr. L. K. POYNTZ, Secretary, 1460 Dallas Road, Victoria, B. C., Canada.

CORRESPONDENCE

Dear Sir:—

In view of the fact that the roentgen-ray and radium treatment of ear conditions appears to be somewhat in the limelight, it is important that previous experiences and experiments be quoted. The following seems to show that experimental investigations on animals have not been made concerning middle-ear infections of the current types. I should like to call attention again to the necessity of such investigations. The following facts seem to be very little known.

Dr. Victor Urbantschitsch, in his "Textbook of Otology," 1910, p. 133, says: "There exist some noteworthy experiments concerning the influence of roentgenization on the diseased middle ear in confined middle-ear suppurations. Schwarz successfully influenced the profuse secretion in a case of tenacious middle-ear suppuration. As Schwarz states, the treatment must be carried on for a long time. It is not necessary to introduce an ear speculum. The whole region of the ear can be rayed, with protection of the face, with fourteen days' intermission and 2 Kalom light power. Joulin (Chronic middle-ear catarrh and otosclerosis. *Arch. d'électic. med.*, 1908, p. 231) cites 10 cases of otosclerosis in which he had remarkable results with roentgenization. Schwarz succeeded in one case among 3 to get almost normal hearing in a sixteen-year-old boy after 6 radiations one month apart. Two qualities of the *x*-ray must be considered, namely, the shrinking effect upon newly formed, especially keloid connective tissue, and further, the arrest of bone formation which has been proved experimentally. According to the experiences so far gathered, it seems that the roentgenization has no damaging influence upon the labyrinthine structures. At least, long-continued treatment with roentgen rays as well as experiments on animals has shown no damaging effects. Contrary to this, a degeneration of Corti's organ can follow treatment with radium.

"Ewald (*Ztschr. f. Physiol.*, 1905, p. 10) found, after application of small glass balls with radium in the ear capsule of pigeons, typical labyrinthine symptoms a few days later, especially turning of the head. Ewald assumes that these symptoms are caused by a paralysis of the ciliated sense-epithelium in the vestibular apparatus. Marx (*Ztschr. f. Physiol.*, lix, p. 102) used radium on the opened ear capsule of pigeons and guinea pigs and found an isolated simple atrophy of Corti's organ. The symptoms in pigeons developed only after six months. The duration of raying had no demonstrable influence upon the latent period." EMIL AMBERG.

February 4, 1922.

PROGRAM OF THE SEVENTH ANNUAL MEETING AMERICAN RADIUM SOCIETY

ST. LOUIS, MO., MAY 22, 23, 1922.

The Seventh Annual Meeting of the American Radium Sodiety will be held at St. Louis, Mo., Monday and Tuesday, May 22nd and 23rd, 1922. The headquarters and place of meeting will be at the Planters Hotel.

The program is as follows:

MONDAY, MAY 22, 1922

Morning Session

(Begins promptly at 9.30)

A. EXECUTIVE SESSION.

B. SCIENTIFIC SESSION.

1. "Lymphosarcoma and other Glandular Enlargements of the Neck: Their Radiation Treatment." EDWARD H. SKINNER, M.D., Kansas City. Discussion to be opened by ALBERT SOILAND, M.D., Los Angeles.

2. "Experience with Radium in the Treatment of Bone Tumors." JOSEPH C. BLOODGOOD, M.D., Baltimore. Discussion to be opened by WILLIAM S. STONE, M.D., New York. (By invitation.)

3. "Treatment of Carcinoma of the Esophagus by Radiation." WALTER MILLS, M.D., and JOHN KIMBROUGH, M.D., St. Louis. (By invitation.) Discussion to be opened by CHARLES F. BOWEN, M.D., Columbus, Ohio.

4. "Statistics and Technique in the Treatment of Carcinoma of the Rectum by Radiation." HARRY H. BOWING, M.D., Rochester, Minn. Discussion to be opened by DOUGLAS QUICK, M.D., New York.

5. "Technique and Ante-Operative Radiation for Carcinoma of the Breast." RUSSELL H. BOGGS, M.D., Pittsburgh.

6. "Statistics and Technique in the Treatment of Carcinoma of the Breast by Radiation, from the Standpoint of the Surgeon." BURTON J. LEE, M.D., New York. (By invitation.)

Discussion on these two papers to be opened by B. R. KIRKENDALL, M.D., Columbus, Ohio.

Afternoon Session

7. "Statistics and Technique in the Treatment of Carcinoma of the Uterus and Pelvis with the Combined Use of Radium and *X*-Rays." HENRY SCHMITZ, M.D., Chicago.

8. "Statistics and Technique in the Treatment of Malignant Disease of the Uterus and Pelvis by Moderate Quantities of Radium." JOHN G. CLARK, M.D., Philadelphia.

9. "Results of Treatment of Carcinoma of the Cervix, with Statistics and Technique." CURTIS F. BURNAM, M.D., Baltimore.

10. "Statistics and Technique in the Treatment of Certain Benign Uterine Conditions by Radium." LEDA J. STACY, M.D., Rochester, Minn.

11. "Statistics and Technique in the Treatment of Uterine Fibroids by Radiation." JAMES A. CORSCADEN, M.D., New York.

Discussion on this symposium to be opened by JAMES T. CASE, M.D., Battle Creek, Mich.

12. "Radium Therapy in Cancer of the Prostate." HERMAN C. BUMPUS, M.D., Rochester, Minn. (By invitation.)

13. "Statistics and Technique in the Treatment of Carcinoma of the Bladder by Radiation." BENJAMIN S. BARRINGER, M.D., New York. Discussion on these two papers to be opened by GEORGE E. PFAHLER, M.D., Philadelphia.

MONDAY EVENING

Annual Dinner. Address by the President, GEORGE E. PFAHLER, M.D., "Protection in Radiology."

TUESDAY, MAY 23, 1922

Morning Session

A. SHORT EXECUTIVE SESSION.

B. SCIENTIFIC SESSION.

14. "Statistics of the Intra-Oral Group of Carcinomas." DOUGLAS QUICK, M.D., New York. Discussion to be opened by REX DUNCAN, M.D., Los Angeles. (By invitation.)

15. "The Treatment of Infected Tonsils by Radium." C. AUGUSTUS SIMPSON, M.D., Washington. (By invitation.) Discussion to be opened by CARL F. ROBINSON, Barre, Vt.

16. "Radium Treatment of Nasal Polyps." H. R. LYONS, M.D., Rochester, Minn. Discussion to be opened by C. EVERETT FIELD, M.D., New York.

17. "Statistics and Technique in the Treatment of Malignant Disease of the Antrum by Radium." D. CROSBY GREENE, M.D., Boston. (By invitation.) Discussion to be opened by HENRY K. PANCOAST, M.D., Philadelphia.

18. "Statistics and Technique in the Treatment of Malignant Neoplasms of the Larynx." DOUGLAS QUICK, M.D., New York. Discussion to be opened by JOHN M. LEE., M.D., Rochester, N.Y.

19. "Technique in the Application of Radium Needles." WILLIAM L. CLARK, M.D., Philadelphia. Discussion to be opened by CHARLES F. BOWEN, M.D., Columbus, Ohio.

Afternoon Session

20. "Statistics and Technique in the Treatment of Malignant Disease of the Skin by Radiation." FRANK E. SIMPSON, M.D., Chicago.

21. "Statistics and Technique in the Treatment of Malignancy of the Skin by Radiation." HOWARD MORROW, M.D., and LAURENCE R. TAUSSIG, M.D., San Francisco. (By invitation.)

22. "The Treatment of Superficial Cancer, with Statistics and Technique." D. T. QUIGLEY, M.D., Omaha.

Discussion on this symposium to be opened by ROLLIN H. STEVENS, M.D., Detroit.

23. "Comparative Measurements between Radium and *X*-Rays Concerning Energy Absorbed at Depth." HARVEY R. GAYLORD, M.D., and DR. CARL W. STENSTROM, BUFFALO. (By invitation.)

24. "On Ionization Measurements." GIOACCHINO FAILLA, E. E., New York.

Discussion on these two papers to be opened by CHARLES H. VIOL, PH. D., Pittsburgh.

25. "Proposed Collective Investigations of Certain Aspects of the Treatment of Malignant Disease." ROBERT B. GREENOUGH, M.D., Boston. (By invitation.)

26. "The Present Field for the Use of Radium and the *X*-Ray in the Treatment of Malignant Neoplastic Disease." WILLIAM S. STONE, M.D., New York. (By invitation.) Discussion on these two papers to be opened by EDWARD C. ERNST, M.D., St. Louis.

27. "The Biological Effects of Radio-Active Substances and *X*-Rays on Cell and Tissue Structures." LEO LOEB, M.D., St. Louis. (By invitation.)

28. "Two Important Points Regarding Cancer Immunity from the Radium Therapist's Standpoint." CARROLL CHASE, M.D., New York.

Discussion on these two papers to be opened by ISAAC LEVIN, M.D., New York.

DEMONSTRATIONS OF APPARATUS, APPLICATORS AND TECHNIQUE

1. CARROLL CHASE, M.D., New York: Illustrating New Chart for Use in Radium Therapy.

2. DR. CARL W. STENSTROM, Buffalo: A New Device for Retubing Radium Emanation.

3. ALBERT SOILAND, M.D., Los Angeles: Lantern Slide Demonstration: The Protection of our Operators from Unnecessary Radium Radiation.

4. SINCLAIR TOUSEY, M.D., New York: Estimation of the Safe Dose of Different Radium Preparations by Photographic Measurements.

5. HENRY K. PANCOAST, M.D., Philadelphia: A Radium Needle Holder for Laryngeal Growths.

TRANSLATIONS & ABSTRACTS

BARJON. Radiodiagnosis of Pleural Symphysis. (*Paris méd.*, February, 1921, ii, 6.)

By symphysis is meant the adhesion of the two laminae of the pleura to each other, which leads to a certain amount of fixation of the lung and thoracic cavity, either total or partial. It is the cicatrix resulting from inflammation of the pleura or lung, and the process of inflammation may be still active or extinct. It is found in the most varied inflammatory and neoplastic conditions, but is itself silent, giving rise to no clinical manifestations. In physical diagnosis it gives rise to diminished resonance and vocal fremitus and lessened to breathing. If the subjacent lung be healthy there will be no reason to suspect pleural adhesion; while if it be diseased the signs of the affected lung tissue mask its presence. In extreme cases of adhesion and cicatricial retraction the thorax is deformed and retracted, with or without deviation of the heart.

Pleural symphysis is often looked upon as a conservative effort of nature rather than as a disease, for it may, for example, prevent the perforation of a cavity. Chiefly in connection with the operation of artificial pneumothorax is it advantageous to know whether the pleural laminae are adherent, for this constitutes a contraindication. Hence the use of radiography before this operation. In the routine use of the rays in the diagnosis of tuberculosis it is also of importance to note the presence or absence of adhesions. In radiographic diagnosis the bases of the lungs are most readily studied for the presence of adhesions. If these are present the sinuses will be effaced while the diaphragm may be fixed.

The author has studied twenty-four cases of adhesions from this double viewpoint. The costo-diaphragmatic sinuses may be completely or only partially obliterated, or normal. In the first case there is seen on the plate total opacity of this portion of the pleural cavity. There may be a certain amount of exudate as a result of the inflammation which has produced the adhesion. This little by little will push back the pulmonary lamina until it is completely dislodged, as a result of which the sinus is the seat of total opacity with loss of its contours. At times the diaphragm loses its convexity, for fixation of the external border to the parieties of the chest has deprived it of mobility. Nearly always in such cases the lung preserves towards its base a certain luminosity, and a clear band is defined immediately above the diaphragm. The opacity of the sinus is due to the fact that the lung has been dislodged from it. It is necessary in these cases to exclude the possibility that the opacity could be due to a lesion of the base of the lung. In pleural exudation the shadow is much more compact and homogeneous with complete obliteration of contours.

If the sinuses appear perfectly normal on the screen we cannot exclude the presence of adhesions, for in twenty-five radioscopies of apparently normal individuals symphysis was later shown to have been present in ten. A positive result on the other hand is conclusive. The importance of immobility of the diaphragm for the diagnosis of symphysis is not great, because this condition occurs under a variety of circumstances. In twenty cases of symphysis the diaphragm was immobilized in but three. The author concludes that the disappearance of the image of the sinus has great and at times pathognomonic value, especially when the symphysis is partial; although negative image does not imply absence of symphysis. The author emphatically denies that with a clear base, normal sinus and mobile diaphragm one may be certain of the success of an artificial pneumothorax; cases undoubtedly exist in which symphysis is present despite these findings.

BONN (Rostock). Roentgen Diagnosis of Acute Mediastinal Abscess. (*Deutsche Ztschr. f. Chir.*, 1920, clviii, 3–4.)

In a paper on the operative treatment of acute mediastinal abscess in which five cases of different types are reported, the author states that while roentgen diagnosis is certain in chronic mediastinal abscess the same cannot be said of the acute type. This he explains by the fact that in tuberculous abscess the shadow is more extensive and better defined, the membrane being thicker. Nevertheless data of value may be obtained in the acute form, although there is no certainty in any recourse save puncture. In one case of the author's there was an indefinite shadow in the region of the third right rib, very difficult to demark from the heart shadow. In another case there was at the height of the aortic arch a round shadow on either side of the vertebral column which was evidently not the shadow of the aorta and aroused suspicion of a small abscess in the posterior mediastinum. In a third case there was a sign of fluid in the right thoracic region the diagnosis varying between interlobar abscess and suppurative mediastinitis.